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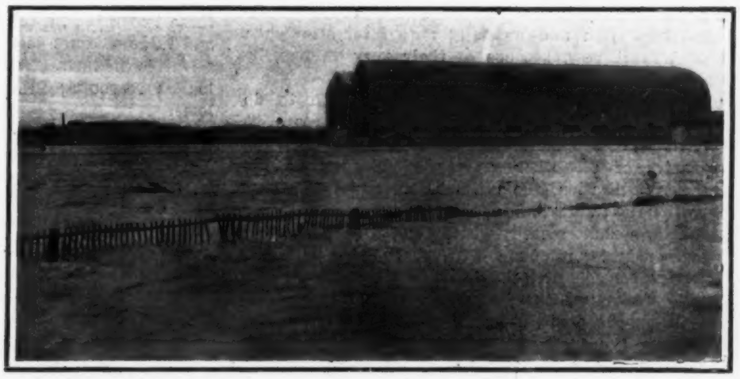
THE GREAT PARIS FLOOD.

PARIS, from the character of the Seine watershed, is constantly exposed to inundation, according to an engineering expert writing in the London Times, but Paris is as heedless of the river as Naples is of Vesuvius or Messina was of earthquakes. Observations have been taken at the Pont de la Tournelle for over two hundred and fifty years. The normal depth of water there is from eight to ten feet; when it reaches twenty feet it causes serious damage. The greatest flood on record was in February, 1658, when a height of twenty-nine feet was recorded. The highest mark we have found for the recent overflow was 8.50 meters, a little less than twenty-eight feet. The Seine scale enables predictions of great accuracy to be made as to what will happen within twenty-four or forty-eight hours, but it provides no means of avoiding what is impending. An attempt to forestall the consequences of the floods has been made by building dikes along the river bank, which are the quais with their retaining walls; but the plans of the engineers were modified. In some cases the walls were not made high enough; in others, as at the Place de la Concorde, great gaps were left open to suit public convenience and æsthetic taste.

The floods are inevitable, though until this year they had not been extremely high for a long time. Some of the streams pouring into the Seine, owing to the nature of their watersheds, become torrents in heavy rains. Others are slower in action, but more dangerous, for they accumulate water till it becomes excessive and then send it in a less rapid but greater body

(Continued on page 136.)

THE FLOODED ESPLANADE DES INVALIDES.



THE WATER INVADING THE AERODROME AT ISSY-LES-MOULINEAUX.

In fighting the progress of the flood the Corps de Génie have shown great energy. All over Paris barrages of bags of cement were built, but in many cases these obstacles to the advance of the water proved ineffectual. A special effort was made to prevent it from entering the Place de la Concorde, and detachments might be seen throwing up barricades of brick, sand, and bags of cement.

By courtesy of Illustrated London News.

THE ONLY MEANS OF EXIT IN THE BOULEVARD HAUSSMANN; RESIDENTS LEAVING THEIR HOUSES THROUGH THE FIRST-STORY WINDOWS.

TWO STREETS IN FLOODED PARIS. THE CITY IS LIKE ANOTHER VENICE.

THE GREAT PARIS FLOOD.

EFFICIENCY IN SHOP OPERATIONS.*

METHODS OF INCREASING IT AND SOME RESULTS SECURED BY THEIR ADOPTION.

BY H. F. STIMPSON.

Managers of industrial enterprises will undoubtedly agree that there are few qualities which are more to be desired in equipment, methods, and men than that of efficiency. From an extensive study of this subject in various parts of the country, together with interviews and correspondence with several hundred concerns, the writer has become convinced that there is a general lack of definite comprehension of what efficiency is, whence it springs, how it may be measured and developed and the results which its cultivation will produce. The object of this paper is an endeavor to throw some light upon these things and to afford a new viewpoint from which to study industrial operations.

THE EVOLUTION OF INDUSTRIAL MANAGEMENT.

In the first place we must realize that the management of industrial enterprises is in a state of evolution. The tremendous growth of the past few years has caused certain previously satisfactory methods to become inadequate to present needs. Many details which in the days of smaller affairs could be absorbed by personal inspection and mentally stored for use when needed must now, because of their very volume, be made matters of record.

The character of these records has much to do with their value. Because financial records are so ancient they have exerted an undue influence upon the character of all other records. While under our present civilization, the ultimate object of industrial operations is to create financial profits, there are many highly important records which cannot be adequately expressed in terms of money. The business of manufacturing consists of a repetition of mechanical operations. Mechanical operations necessarily involve considerations of weight, distance, time and effort, but not of money.

The reason for the failure of so many cost systems to serve the desired end is that they are based upon a wrong unit. These systems become useful only beyond a certain point. Other systems have been the result of a blind craving for aid, but being without broad underlying principles and not properly tied together and simply, in many cases, disjointed attempts to improve isolated details, they too have failed. The result is that attempts by specialists to improve industrial conditions have been often looked upon with suspicion and this is not altogether without reason. These very failures, however, have drawn the attention of men in certain lines of engineering to the rapidly developing needs of manufacturers. They have attempted to solve the problems by the use of engineering instead of by accounting methods, and the results which have been attained prove conclusively that a material advance has been made.

WHAT IS EFFICIENCY?

With this understanding of the present conditions, let us consider what efficiency really is. It has been defined as "the ability to produce certain results," and this at the very outset necessitates the existence or creation of a standard of measurement. Our perception of efficiency, therefore, is correct only in proportion to the precision of the standard, which must be accurately developed from data which are not only exact, but complete. A machinist, believed to be operating at high efficiency, was observed while turning a shaft. His cut, feed, and speed seemed to be beyond criticism. When the shaft was finished, however, he had to spend half as much time in hunting up a chain and pad to remove the shaft from the lathe, as he had taken in turning it. This cut his actual efficiency from 100 per cent down to 87 per cent; yet the man was not at fault. His normal work was to operate a lathe and not to hunt for things which should have been provided for him. The points to be observed here are not only the importance of using the right standard of measurement, but that the efficiency of the man depended very largely upon his surrounding conditions over which he had no control. These conditions depend upon the efficiency of the management in securing proper equipment from the owners. This in turn depends upon the efficiency of the management's records in enabling it to state clearly and accurately what increase in output and consequently in profits will result from improving the conditions—thus justifying the expenditure required. We see from this that the true standard is not the possibility under existing conditions, but that which can be obtained under other and more desirable conditions.

MANAGERIAL OPPOSITION TO CHANGE.

The management, which immediately controls the records and conditions, should be the prime source of efforts toward the increase of efficiency throughout the plant. The opposition of managers to progress in this respect is exceedingly great, yet not altogether surprising, for these reasons:

1. There is a widespread fallacy that so-called practical experience in the manual operations or technical processes of a business is the chief essential to success in its management. This is due to the fact that perfection of workmanship, of which he knows much, is more important in the eyes of the artisan than the actual cost of the operation, of which he knows little, or than the cause of this cost, of which he knows less.

2. It is only recently that educational institutions have afforded any opportunity for adequate instruction in the art of management, pure and simple, a principal feature of which is the intelligent regulation of cost.

3. There has been, and now is, as a result of these two things, a failure to appreciate the necessity and value of exact data in proper terms, of refined and scientific methods of collecting and using them and of logical reasoning in the solution of industrial problems.

The highest degree of efficiency, therefore, is only to be realized in a shop where executive methods have reached a high stage of efficiency, for in these is unquestionably its source.

TIME MEASUREMENTS IMPORTANT.

The first step is to recognize the necessity and value of a proper measurement of time, as a guide not only to the executive but to the workman. A man was observed during eight successive repetitions of the operation of making a machine mold in a foundry. The unit times varied from 5.2 to 23.6 minutes, the total time for the eight being 104 minutes. Under the method of timekeeping in use at that shop it was only ascertained that the eight operations took 1¾ hours or an "average" of 13 minutes each, and the labor cost and distribution of burden were made on that basis. Because of the absence of any standard time whatsoever it was not realized that had the man done each of the eight in 5.2 minutes, they would have been completed in 41.6 minutes, resulting in a saving of over 60 per cent of the total time. Had the man received a proper work ticket bearing this standard time, before he began the work, there is no doubt that he could have easily performed the work in the shorter time, and a marked difference in proportionate burden and cost would have resulted. Under the existing methods the management could not know of the waste, and so was helpless to prevent or cure it.

Every item of time, therefore, is capable of division into two parts: A standard or necessary time and a (more or less) preventable waste, which latter is the easier thing of the two to determine.

AN EXAMPLE OF INCREASED EFFICIENCY IN RIVETING.

A gang of four were engaged in riveting some steel plates. By the use of a stop-watch, it was found that a large proportion of the total time of the riveter and bucker-up was not utilized; yet some one was always at work. The reason was that the men proceeded along the work in such a way that the bucker-up covered with his body the holes as yet unfilled by rivets, he moving from left to right. When, therefore, a rivet was driven, these two men had to stand aside until another rivet was placed by the rivet-passer. Upon the instruction of the engineer, they reversed the direction of their movements so as to cover only the filled holes, thus enabling the passer always to have a rivet ready for them and making their speed in driving the real gage of the speed of the operation. Furthermore, when they encountered a hole that needed reaming (as was sometimes the case, until the fault was located with the fitters and remedied), the riveter would lay down the gun, pick up the reamer, ream the hole, lay down the reamer, pick up the gun and drive the rivet. When persuaded to test consecutively ten or more holes after driving the first rivet in a seam to anchor the plates and then to drive the ten consecutively, they progressed faster with less effort. These men, receiving not only a standard from the engineer, but kindly instruction as to how to attain it, and being stimulated, not by abuse, but by a scientifically determined bonus, increased their output over 150 per cent beyond the original amount.

In this plant, by the use of these methods, and in about seven months, the general increase in efficiency of the men was such that the force was reduced 67 per cent without reduction in volume of output, but with a great reduction in net total unit cost, even after paying the bonus alluded to and the cost of the expert services which alone produced this result.

THE USE OF BONUSES.

It is proper to say a word here on the subject of bonus as a means of increasing efficiency. The principal merit of this motive lies in the fact that immediate personal gain is the strongest incentive to immediate personal effort. It operates just as strongly on the employee as on the employer. Hope of promotion is too vague and the actual chances too limited to exert much pressure, but an extra sum in the pay envelope—or better still, in a separate one—for the disposal of the "old man himself," will do wonders. To be most effectual a bonus must begin at the point of standard efficiency, but at the point when average efficiency ceases and extra effort begins; and it should increase on a curve faster and faster as the point of standard efficiency is neared, because the accompanying effort will be correspondingly greater.

EFFICIENCY METHODS AND DEPARTMENT HEADS.

So much for the individual operator. And now for the executives. From foremen up to and including the highest official the same methods can and should be applied. Under ordinary circumstances, the workman in need of material, tools or instruction keeps his skirts clear by a more or less indefinite and unintelligible request to the foreman. He thinks it the foreman's duty to look after him, but that if he does not do so it's no business of his. Put that man on standard time and bonus and if there is anything he thinks the foreman should do or get for him he speaks loudly and directly. This the foreman does not resent—as would ordinarily be the case—for his efficiency is determined by the combined efficiency of his men and upon this his bonus depends. Anything, therefore, that interferes with the progress of the men touches him closely, and he will move heaven and earth to eliminate it. All kinds of defects which were previously hidden from the superintendent are now brought to his attention, and he welcomes them for exactly the same reason that actuated the foreman. Thus the change that comes over a shop when efficiency is accurately measured and adequately rewarded is often astonishing.

But this is not all. The possession of exact data as to standard and actual times makes possible a certain great improvement in, and addition to, the executive staff and a material increase in the efficiency of the foremen and department heads. By this is meant the installation of a planning department, by which the apportionment of the time of men and machines is controlled.* The advantage, indeed, the positive necessity, of the service of engineers and draftsmen in apportioning the different parts of the product is well understood. The requirements of each part, the strains to which it will be subjected, the kind, quality and quantity of material required to resist these strains, the shapes of the pieces, their relations to each other and many other things are all given most careful attention. The value of fully constructing the design on paper, as a means of discovering possible errors or difficulties, and of correcting or overcoming them before large expense for material and workmanship has been incurred, are too well realized to need more than a simple statement for their acceptance. No sane executive would expect his department heads to take a copy of his customer's order and individually work out the details with which they are particularly concerned and expect the parts to fit. Yet this is just exactly what is being done as regards the apportionment of productive time; and a tumult of broken promises of delivery, excessive costs of production, enormous wastes of time in changing jobs, etc., is the immediate and unavoidable result.

WHAT CAN BE DONE?

It is perfectly possible, but only to one trained in the particular art, to schedule the different operations on all of the different parts of the product; to plot the productive times required, so that each may begin at such a time in relation to the others that all will arrive at the point of assembly at the proper time and in the proper sequence; to combine these studies of the different productive orders on a chart which will show the disposition to be made of all the men and machinery; to prepare advance programmes for each man and machine engaged in productive labor; and thus to give to the superintendent and foreman the

* The Iron Age.

* See "Graphical Helps for Apportioning Time in Constructive Operations," Engineering Magazine, September, 1909.

advantage of the same predisposition of time that they now have of material.

As it is now, the time of these persons is entirely too much occupied with this problem of the disposition of time for which they are only partly equipped, having, it is true, much of the necessary information, but no training in the scientific handling of it. They are, therefore, unable to devote the time they should to the immediate study of the operations and the provision of tools, material, and instruction to the men. They try to be all over the shop at once and they depend on getting their information at first hand, and consequently fail more or less clearly to cover the ground. Having such schedules and programmes as are above described, and with the proper work tickets distributed on a dispatching board, each one in the division representing the work upon which a man or machine is engaged, having the time of commencement and the standard time thereon, the foreman can see at a glance without leaving his office what men will shortly finish their work and what steps must be taken to see that the drawings, tools, and materials for their next work are ready for them in time. Having seen to this he has some leisure to give his attention to matters immediately requiring it, knowing if anything is obstructing the other men, that their anxiety to earn their bonus will cause them promptly to bring such matters to his attention. Having this schedule, moreover, the foremen are enabled to order material, etc., ahead and to do so intelligently, thus making the work of the shop transportation department much simpler. In one case by this means 25 men were able to handle the intra-shop transportation in a more satisfactory manner than 75 men had previously been able to do.

The planning department also greatly aids and is in turn aided by the purchasing department, for the times when material must or can be got can intelligently be determined to their mutual advantage. The sales department, too, when it once gets the idea that the shop is not working miracles, but has its limitations, can make delivery promises which really mean something and can be kept, and this is a trump card of no small importance when the fact becomes realized among the customers of the concern.

RESPONSIBILITY OF THE MANAGEMENT.

In the opinion of those whose opportunities have enabled them to get the facts, the inefficiency in manufacturing which undoubtedly generally exists today, in spite of the prevailing impression to the contrary, is only about one-fourth due to the things over which the employees have control and three-fourths to conditions imposed upon them by the management. The methods outlined above have achieved results whenever they have been faithfully and honestly tried, with proper co-operation by the management and under the direction of skilled specialists, and the results have continued and will continue as long as the methods are followed. The effect upon the men is that from being often listless, indifferent and antagonistic, they become energetic, ambitious, and loyal friends.

One thing more: Much has been done and overdone in the line of so-called welfare work. It is a highly creditable and necessary line of effort, when confined to attempts to remove from the path of the employee any obstacle which prevents him from developing his skill and efficiency to the highest degree. An uncomfortable, unhappy person cannot be efficient. But as steam is necessary to the engine, so is incentive necessary to the worker to get him to make the best use of the facilities provided for him. Under our present civilization, the same incentive which pushes on the master will push on the man, and that is direct personal gain in dollars and cents, not for himself, but for and what that gain will bring. It must come to him quickly after the exertion which its expectation calls forth, for if long delayed, the effect is lost. It must also come to him separately from his regular wage that its amount may be the more readily realized.

Moreover, the results of efficiency methods, within the writer's knowledge, are sufficient to convince him that their general adoption would so increase the purchasing power of the employee, by increasing his wages and decreasing the cost of production, as to have a markedly beneficial and steadying effect upon the business of the country.

Efficiency methods, however, cannot be successfully designed or installed by those trained in other lines and prejudiced by other associations. After these methods have been scientifically developed to suit the existing conditions and actually put into operation by those skilled in the art, they may gradually be relinquished into the control of those who have been educated in the process of installation, with some hope of success for their future operation.

THE PIONEER OF AERIAL FLIGHT.*

THE WORK OF SAMUEL PIERPONT LANGLEY.

BY ALEXANDER GRAHAM BELL.

Who are responsible for the great developments in aerodromics of the last few years? Not simply the men of the present, but also the men of the past.

To one man especially is honor due—our own Dr. S. P. Langley, late secretary of the Smithsonian Institution. When we trace backward the course of history, we come unfailingly to him as the great pioneer of aerial flight.

We have honored his name by the establishment of the Langley medal; and it may not be out of place on this, the first occasion for the presentation of the medal, to say a few words concerning Langley's work.

Langley devoted his attention to aerodromics at a time when the idea of a flying machine was a subject for ridicule and scorn. It was as much as a man's reputation was worth to be known to be at work upon the subject. He bravely faced the issue, and gave to the world his celebrated memoir entitled, "Experiments in Aerodynamics."

In this work he laid the foundation for a science and art of aerodromics, and raised the whole subject of aerial flight to a scientific plane.

The knowledge that this eminent man of science believed in the practicability of human flight gave a great stimulus to the activities of others, and started the modern movement in favor of aviation that is such a marked feature of to-day.

Everyone now recognizes the influence exerted by Langley on the development of this art. The Wright brothers, too, have laid their tribute at his feet.

"The knowledge," they say, "that the head of the most prominent scientific institution of America believed in the possibility of human flight was one of the influences that led us to undertake the preliminary investigations that preceded our active work. He recommended to us the books which enabled us to form sane ideas at the outset. It was a helping hand at a critical time, and we shall always be grateful."

Langley's experiments in aerodynamics gave to physicists, perhaps for the first time, firm ground on which to stand as to the long-disputed questions of air resistances and reactions. Chanute says:

(a) They established a more reliable coefficient for rectangular pressures than that of Smeaton.

(b) They proved that upon inclined planes the air pressures were really normal to the surface.

(c) They disproved the Newtonian law that the normal pressure varied as the square of the angle of incidence on inclined planes.

(d) They showed that the empirical formula of Duchemin, proposed in 1836 and ignored for fifty years, was approximately correct.

(e) That the position of the center of pressure varied with the angle of inclination, and that on planes its movements approximately followed the law formulated by Joessel.

(f) That oblong planes, presented with their longest dimension to the line of motion, were more effective for support than when presented with their narrower side.

(g) That planes might be superposed without loss of supporting power if spaced apart certain distances which varied with the speed; and

(h) That thin planes consumed less power for support at high speeds than at low speeds.

The paradoxical result obtained by Langley that it takes less power to support a plane at high speed than at low, opens up enormous possibilities for the aerodrome of the future. It results, as Chanute has pointed out, from the fact that the higher the speed, the less need be the angle of inclination to sustain a given weight, and the less, therefore, the horizontal component of the air pressure.

It is true only, however, of the plane itself; and not of the struts and framework that go to make up the rest of a flying machine. In order, therefore, to take full advantage of Langley's law, those portions of the machine that offer head resistance alone, without contributing anything to the support of the machine in the air, should be reduced to a minimum.

After laying the foundations of a science of aerodromics, Langley proceeded to reduce his theories to practice.

Between 1891 and 1895 he built four aerodrome models; one driven by carbonic acid gas and three by steam engines.

On the 6th of May, 1896, his aerodrome No. 5 was tried upon the Potomac River near Quantico. I was myself a witness of this celebrated experiment, and secured photographs of the machine in the air, which have been widely published.

This aerodrome carried a steam engine, and had a spread of wing of from 12 to 14 feet. It was shot into the air from the top of a house boat anchored in a quiet bay near Quantico.

It made a beautiful flight of about 3,000 feet, considerably over half a mile. It was indeed a most inspiring spectacle to see a steam engine in the air flying with wings like a bird. The equilibrium seemed to be perfect, although no man was on board to control and guide the machine.

I witnessed two flights of this aerodrome on the same day; and came to the conclusion that the possibility of aerial flight by heavier-than-air machines had been fully demonstrated. The world took the same view; and the progress of practical aerodromics was immensely stimulated by the experiments.

Langley afterward constructed a number of other aerodrome models which were flown with equal success, and he then felt that he had brought his researches to a conclusion, and desired to leave to others the task of bringing the experiments to the man-carrying stage.

Later, however, encouraged by the appreciation of the War Department, which recognized in the Langley aerodrome a possible new engine of war and stimulated by an appropriation of $50,000, he constructed a full-sized aerodrome to carry a man.

Two attempts were made, with Mr. Charles Manley on board as aviator, to shoot the machine into the air from the top of a houseboat; but on each occasion the machine caught on the launching ways, and was precipitated into the water. The public, not knowing the nature of the defect which prevented the aerodrome from taking the air, received the impression that the machine itself was a failure and could not fly.

This conclusion was not warranted by the facts, and to me, and to others who have examined the apparatus, it seemed to be a perfectly good flying machine—excellently constructed, and the fruit of years of labor. It was simply never launched into the air, and so has never had the opportunity of showing what it could do. Who can say what a third trial might have demonstrated? The general ridicule, however, with which the first two failures were received prevented any further appropriation of money to give it another trial.

Langley never recovered from his disappointment. He was humiliated by the ridicule with which his efforts had been received; and had, shortly afterward, a stroke of paralysis. Within a few months a second stroke came, and deprived him of life.

He had some consolation, however, at the end. Upon his deathbed he received the resolutions of the newly formed Aero Club of America, conveying the sympathy of the members, and their high appreciation of his work.

Langley's faith never wavered, but he never saw a man-carrying aerodrome in the air.

His greatest achievements in practical aerodromics consisted in the successful construction of power-driven models which actually flew. With their construction he thought that he had finished his work; and in 1901, in announcing the supposed conclusion of his labors, he said:

"I have brought to a close the portion of the work which seemed to be specially mine—the demonstration of the practicability of mechanical flight—and for the next stage, which is the commercial and practical development of the idea, it is probable that the world may look to others."

He was right, and the others have appeared. The aerodrome has reached the commercial and practical stage; and chief among those who are developing this field are the brothers Wilbur and Orville Wright. They are eminently deserving of the highest honor from us for their great achievements.

* Abstracted from an address delivered at the presentation of the Langley Medal to the Wright brothers, February 10th, 1910.

SOME EXPERIENCES OF AN AVIATOR.

HOW ONE MAN LEARNED TO FLY WITH A BLÉRIOT.

BY CLAUDE GRAHAME-WHITE.

IN the beginning of 1908 my enthusiasm was aroused in aeroplanes, and I immediately set to work to construct a model helicopter, which at that time presented to me the best means of flying, being a type of aeroplane that would rise from the ground immediately without any speed impetus in a forward direction. I constructed a couple of models on this principle, but soon discarded this form of aeroplane, for I found that a tremendous power was required to lift the dead weight off the ground without attaining a forward velocity, and although in theory the helicopter presented many advantages, I found that in practice they were thoroughly outbalanced by the over-numerous disadvantages.

Toward the middle of the same year I on several occasions visited the works of Voisin Frères at Billancourt, in the environs of Paris, and derived considerable valuable information in connection with biplanes from M. Gabriel Voisin. I, however, always have had a penchant toward monoplanes, which type of flying machine, to my mind, more closely approximates the natural flight of a bird.

I then set to work to ascertain which was the best type of monoplane at that time being manufactured, and although I accumulated a good deal of knowledge, it was not until M. Blériot successfully crossed the Channel on his monoplane in July last that I seriously took up aeroplanes commercially. Since that date I have devoted the whole of my time and attention to the subject, and have visited all the various aeroplane meetings which have been held on the Continent, and as a result of the experience I have thus gained, my choice finally fell on a Blériot type monoplane, several of which I placed orders for in August last. I secured absolutely the first delivery of the big "Type XII." Blériot monoplane, which differs very essentially from the "Type XI." in which M. Blériot crossed the Channel.

The last five weeks I have spent daily at M. Blériot's works in Paris, in order to thoroughly acquaint myself with the construction and various essential points in connection with the erecting of a successful machine. My monoplane was due for delivery at the end of September, but, owing to M. Blériot's special request, I sanctioned its being exhibited at the first aeroplane exhibition at the Grand Palais in Paris, where it was on view from September 25th to October 17th, which naturally very considerably retarded the date of delivery to me.

At the closing of the exhibition the machine was removed to the E. N. V. motor works, which firm fitted the 60-80 horse-power eight-cylinder "V" type water-cooled engine, which has been the means of all the various very successful flights accomplished by Rougier in his Voisin biplane. I did not decide on this engine until I had visited the works of practically all the other aerial motor manufacturers, but after exhaustive research and trials I became convinced that my choice of an E. N. V. engine was of the best.

There are innumerable construction features in this engine which immediately recommend themselves to the novice, inasmuch as while weight has been reduced to a minimum, the essential and vital working parts have been kept well over the margin of strength to eliminate failure as far as possible from breakdowns. These engines are water-cooled, and the copper water jackets are electrolytically deposited on the cast-iron cylinders in the most ingenious manner, thus obviating all packed joints, and the consequent leaking and loss of water, which may prove most disastrous when the machine is in actual flight.

There is one central cam-shaft, which is fitted with seventeen cams, sixteen of which operate the valves, while the remaining one operates, by means of a bell crank lever, a plunger oil pump, which forces the oil through the hollow crankshaft to the big-end bearings, and thence by suitable piping to the cam-shaft, pistons, and gudgeon pins. The lubricating oil is fed to the crank chamber by gravity, and is kept at a constant level in the crank chamber by an ordinary float, a downward movement of which opens a by-pass to the oil tank, which is automatically closed again by the float when the oil reaches its normal level.

The magneto and pump are both placed on top of the engine, and are driven by a short shaft, and are very get-at-able, and easily dismounted. The crankshaft is fitted with ball bearings throughout, and this system appears to be highly satisfactory. The carburetor is placed in the center of the engine, between the cylinders, and, by means of an ingenious arrangement of copper piping, the vapor is collected in a center dome, which insures equal distribution of the gases to the various cylinders, and great economy in the weight of piping. The cylinders are 100 millimeters bore by 130 millimeters stroke, and 60 horse-power, the normal power of the engine, is developed at 1,000 revolutions, and at 1,500 to 1,600 revolutions 80 horse-power. The motor weighs—complete with accessories, and coil and accumulator for dual ignition—just over 300 pounds.

As will be noticed from the accompanying illustration, the design of the machine differs very essentially from the small Blériot type, Calais-Dover, inasmuch as the engine is placed very much lower in the fusilage, and the driver's and passenger's seats are placed behind the engine in the same plane, thereby bringing the center of gravity very much lower, insuring greater stability in flight.

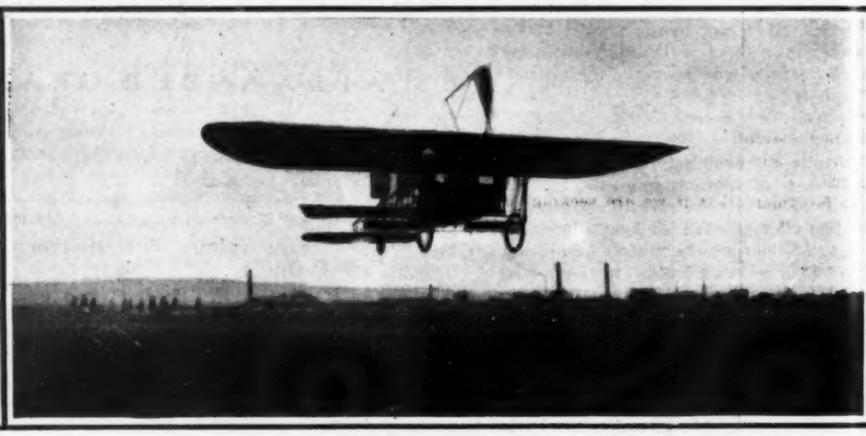
The propeller shaft is fixed high up in the extreme front of the machine, and the propeller, which has a diameter of 2.4 meters, is chain-driven, and is geared down to revolve at half the speed of the engine, for, as precisely in the same manner for marine use, the lower the revolutions of the propeller the greater its efficiency, there being a greater grip of the air and less slip.

After the *mise au point* of the motor and a six hours' continuous run, to test radiator efficiency and the satisfactory working of the pressure feed and all other details, the machine was removed to Blériot's works to have the final adjustments of the wings, the vertical plane, and the fixed stabilizer, rudder, and elevator; and, after several disappointments, I eventually obtained delivery of my machine in working order at the Parade Grounds at Issy-les-Moulineaux, at 7:30 on Sunday morning, November 7th; and, as I had gathered a good deal of useful knowledge and information from watching the antics, and profiting by the errors, made by other beginners on Blériot monoplanes, I had a good idea of what not to do when the engine was started up and we were ready for our first trial. As M. Blériot was away in Vienna himself, and none of his employees had ever actually flown a machine we had a more or less hazy idea as to how it should be controlled.

It was a very cold morning, but the engine started up at the first quarter turn. After many warnings from M. Blériot's foreman, who was in charge, that I was not on any account to accelerate my engine too much, I mounted the machine along with my friend as passenger, and immediately gave word to let go, and we were soon speeding along over the ground at a good sixty kilometers an hour, but with a sense of perfect control and safety, and the faster we went the less inconvenience we found from the draft caused by the propeller. Being very anxious to see whether the machine would lift off the ground, I gave a slight jerk to the elevating plane, and soon felt the machine lift off the ground; but, remembering the warnings of the foreman, and being anxious not to risk breaking the machine, I closed the throttle, and contented myself by running around the ground in order to familiarize myself with the handling of the machine.

As no flying is allowed after eight o'clock on Sunday morning at Issy, we only had half an hour's practice, and, highly satisfied with my first experience, we started off to Mourmelon by car, and, after a 200-kilometer run, we arrived there in time to see Mr. Farman conduct some very interesting flights with a passenger, which, just at nightfall, resulted most disastrously. Owing to the breaking of one of the wires which operated his elevator, he was unable to move it, and the machine kept ascending against his will. In order to avoid a more serious accident, he had to shut off his motor, and hundreds of spectators present were horrified to see the machine fall like a stone from a height of 60 feet.

MR. C. GRAHAME-WHITE AND FRIEND ON HIS BLÉRIOT No. 12 TYPE MONOPLANE.



A SUCCESSFUL FLIGHT WITH THE NEW TYPE OF BLÉRIOT MACHINE.

I happened to be on my car at the time the accident took place, which was about a mile from Mr. Farman's hangar, from where I was watching his flights. I immediately set out on the car with some of his mechanics, and arrived first on the spot, to find Mr. Farman and his passenger absolutely unhurt, but the machine smashed to pieces.

I took a late train back from Mourmelon on Sunday night, and arrived in Paris at 1 A. M. on Monday morning. Then after three hours' rest my friend and I turned out again, and got down to Issy-les-Moulineaux, about five o'clock in the morning, arriving some two hours before Blériot's mechanics turned up. We, however, got the machine out and tied it to some railings, and I then had my first experience of starting the engine, which to a novice at first sight appears a most hazardous undertaking, as unless the machine is either held firmly by several men, or strongly tied up, it immediately has a tendency to leap forward. We, however, successfully managed to start the engine, and then rigged up a leash, and when we had mounted the machine we let go, and before eight o'clock in the morning we had accomplished several very successful flights, both with and against the wind. These experiences we continued throughout the day, and by nightfall I felt quite capable of an extended flight if only the ground had been large enough.

The following day we were out again early, and had absolutely no difficulty in flying the machine with my friend as passenger and sixty liters of gasoline and twenty liters of lubricating oil. We, however, had a somewhat unfortunate accident toward mid-day. Another small Blériot had become damaged, and was stopped some two hours in the center of the field, which much handicapped my maneuvers, and at one time, when we were at a height of 25 feet, my whole attention appeared to be absorbed by this stationary

machine in the middle of the practice ground. In my over-anxiety to avoid going in its direction I accelerated my motor instead of decelerating as was my intention. The machine then rose very much higher in the air than I had any intention to do, and seeing that I should have to fly right over the stationary machine if I continued in my course, I decided to make a hasty descent. This I successfully accomplished in ample time before reaching the other machine, and on grounding it was my intention to stop my motor. I pulled the throttle toward me for this purpose, being accustomed to decelerate motor cars in this manner, but instead of closing the throttle by this action I opened it, and, of course, accelerated the speed of my engine so quickly that it caused the tail of the machine to turn right round, in the same manner as a car would skid if the brakes were jammed on quickly on a greasy road. The severe side strain of this movement caused one of the front forks of the front wheels of the machine, which carry all the dead weight of the engine and passengers, to twist and break, and this put the machine *hors de combat* for the rest of the day.

In order to give some idea of the weight of the machine, I may mention that it took eight strong men to lift one side of it in order to wheel it back into the hanger. The machine in working order, with gasoline and lubricating oil for a three hours' continuous flight, weighs between 900 and 1,000 kilogrammes, or about a ton.

The following day M. Blériot had returned from Vienna, and he sent for me and strongly urged me not to use the aeroplane any more at Issy, as he said the ground was far too small for such a powerful and fast machine, and he further told me that he himself would not attempt to fly it there, and strongly advised me to send it off immediately down to Pau, where there is a very fine aerodrome with hangars and every other convenience for aeroplanists.

I therefore immediately had the machine packed up, and it is now on its way down to Pau, and I propose joining the machine there with M. Blériot early next week, he having undertaken to personally instruct me and to make me thoroughly proficient in the management of the machine before bringing it over to this country.—The Car.

EXPLOSIVES FOR USE IN COAL MINES.*

HOW DYNAMITE IS THAWED SAFELY.

BY C. E. MUNROE AND CLARENCE HALL.

A large number of different explosives can be formed by mixing various combustible substances with various oxidizing agents, or by using such mixtures as "dopes" for dynamite, or by using them together with the different nitro-substitution compounds. In fact, the number is so great that a book published in 1895 gave the names of more than one thousand different explosives, and many have been added to the list since that date. As the number of those in actual use is much smaller, it is clear that most of the explosives known are, for various reasons, unsuitable for use; indeed, not one of those more generally known is suitable for use under all circumstances. For instance, some of them on explosion give off a considerable volume of flame; some of them on explosion give off considerable volumes of poisonous or noxious gases; some of them explode so quickly as to shatter the rock or other material in which they are fired and break it down into fine pieces; some of them are too bulky, and so on.

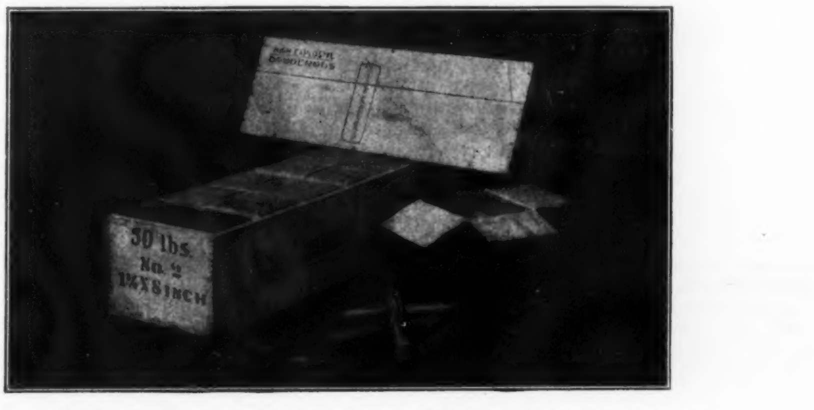


Fig. 2.—Boxes of Dynamite, Showing Method of Packing It.

It is evident that if we are seeking to break down the rock in an open quarry for the purpose of making use of that rock as ballast for roads, we can employ an explosive that produces a long flame, or gives off poisonous gases, or shatters the material, because the work is done in the open air, and because the rock has to be broken up into small pieces anyway, so that it is well for the explosive to do it. If, however, we sought to get out from that quarry blocks of stone such as marble or granite, which were to be used in building or for monuments, we should avoid using the shattering explosive and choose one that slowly, and without a shock, separates the rock mass from the deposit. Yet for this purpose also, as the work is in the open air, it might matter little if the explosive gave rise to a large flame or gave off poisonous gases, provided the quantity of explosive used was small. But the conditions in a coal mine are very different from those in a quarry. The mine is inclosed and not out in the open air, and, moreover, it is liable at any time to contain inflammable gases or coal dust, or both, which may form explosive mixtures with the air. If under these circumstances an explosive were used which gave off a long flame on firing, this flame, darting out from the bore hole, might set fire to the explosive mixture in the mine and produce a mine explosion.

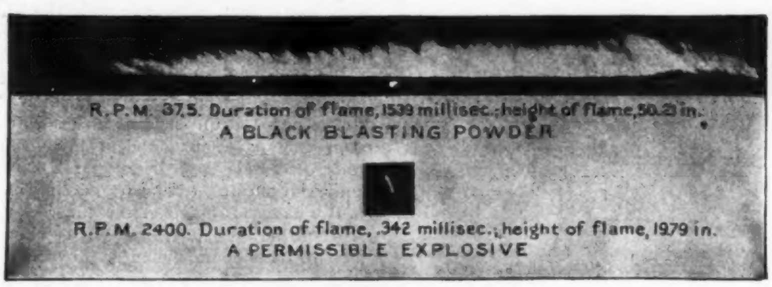


Fig. 1.—Flames from Explosion of Equal Weights of Black Blasting Powder and of a Permissible Explosive.

Moreover, as the mine is an inclosed space and as the wholesomeness of the air within it depends upon artificial ventilation, it is also objectionable in such a mine to make use of an explosive giving off any considerable quantity of noxious gases, which must be removed from the mine after the explosion before the miner can safely return to his work.

Considerations such as these have led to an investigation by the United States Geological Survey of the explosives offered in the market, for the purpose of determining, in the interest of the coal miner, which of such explosives are suitable for use in coal mines and will do the work with the greatest degree of safety for the miner using them.* Of course, it is impossible to insure complete safety, for no explosive can be perfectly safe and every explosive should be treated with the greatest care and consideration.

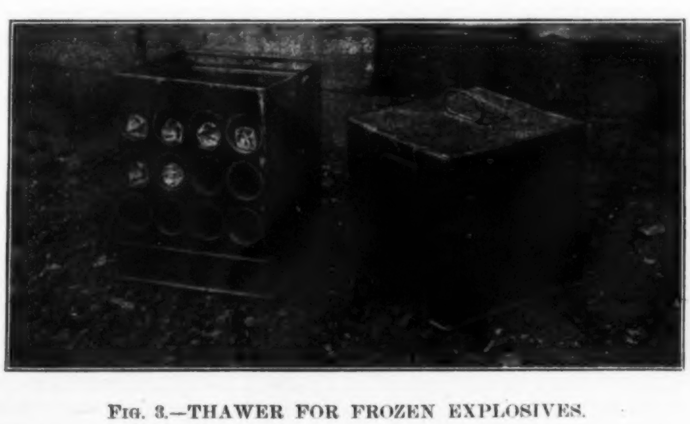


Fig. 3.—Thawer for Frozen Explosives.

With this object in view the United States Geological Survey has established a testing station at Pittsburg, Pa., where there has been erected a large gallery made of steel, 100 feet long by 6 feet 4 inches in diameter, and so made that it can be filled with fire damp, or with coal dust and air mixed, or with gas, dust, and air mixed in any desired proportion, so as to reproduce the dangerous conditions that may occur in coal mines. Attached to one end of the gallery is a very strong "gun" with a chamber representing the bore hole in a mine. From this "gun" various exactly determined quantities of the explosive to be tetsed may be fired, either untamped or tamped, into the mixture in the gallery, and thereby it can be learned whether or not the definitely known quantity of explosive used as described will cause the ignition and explosion of the mixture in the gallery. Besides this gas and dust gallery there is at the testing station a collection of other apparatus and appliances by which to test the various properties of explosives, and so to find their relative value and their relative safety for use in coal mines. This investigation has been going on since September, 1908, and there have already been published two lists of explosives that have passed all test requirements in a satisfactory manner and are considered to be suitable for use in coal mines, provided they are used under the prescribed conditions.

These approved explosives are, therefore, called by the United States Geological Survey "permissible explosives."

With reference to the characteristic component of each of these permissible explosives, they may be placed in three classes—ammonium nitrate powders, hydrated powders, and nitroglycerin powders. Most of these powders contain nitroglycerin, and therefore

* Abstracted from Bulletin 423, published by the United States Geological Survey.

they are all of the general nature of dynamite; but the components and proportions of the "dope" have been so chosen and the mixtures so made as to modify very greatly the shattering effect upon explosion, while at the same time the volumes of gases produced are relatively cool. As a result, the flames produced are short and not lasting, and coal is thrown out without being powdered, when the proper charge of such an explosive is used and this charge is properly placed. These explosives are designed to take the place of black blasting powder, which has been found to be unsuited for use in coal mines where dangerous gas or inflammable dust is present, because of the great mass of flame which it produces and the long time that this flame lasts, and because of the quantity of poisonous smoke and noxious gases which it gives out when exploded. Although explosives of the kind represented in the list of permissible explosives have been introduced into this country only during the last few years, yet their consumption now amounts to several million pounds annually and is rapidly growing.

Unfortunately, though the permissible explosives are good coal getters and yield short flames that do not last and gases of low temperature, so that there is little danger of igniting the explosive gases and dusts in mines when they are used, still the gases they yield on explosion may be noxious and inflammable; and miners are warned when using permissible explosives, not to return to the breast after firing them any sooner than they would if they had fired a charge of black blasting powder.

Most of these permissible explosives will freeze. It is true that in making several of them materials have been added that prevent their freezing so readily, but the manufacturers do not claim that such explosives will remain unfrozen when the temperature falls below 35 deg. F. To keep any of the nitroglycerin explosives permanently thawed, they should be stored where the temperature does not go below 52 deg. F. On the other hand, care should be taken that none of these explosives is subjected to high temperatures, for this will render all of them more sensitive to explosion and is likely to cause the decomposition of some of them. It is best that the temperature of magazines in which they are stored should not rise above 90 deg. F.

Explosives should not be exposed for any length of time to direct sunlight, because this may lead to decomposition in those containing nitroglycerin, nitrocellulose, nitrostarch, or substances of that kind. Explosives should be stored in a dry place, for many of them contain considerable quantities of ammonium nitrate or of sodium nitrate and so will take up moisture from damp air and become damp. Too great dampness makes the explosive not any harder to fire, but weaker when fired. Besides, if the explosive is damp the nature of the gases produced will be different. Moreover, as bodies like dynamite become moist, the nitroglycerin contained in them tends to run out; that is, what is called exudation takes place, and all the dangers follow that belong to liquid nitroglycerin.

On the other hand, explosives should not be kept in an extremely dry place, for all of them, as made, contain some moisture, and if the place of storage is very dry the explosive may lose this moisture. Such a change in composition will affect the explosive so as to change the speed with which the explosive reaction takes place within it, and therefore the character of the work which it does when exploded. Naturally, the longer an explosive is kept in storage the greater are the chances that change will take place in it, and therefore the explosive should be obtained in as fresh a condition as possible and should be used as soon as possible after it is received. Also, it should be kept stored in its original packages in the magazine outside the mine until wanted for immediate use, and then used promptly.

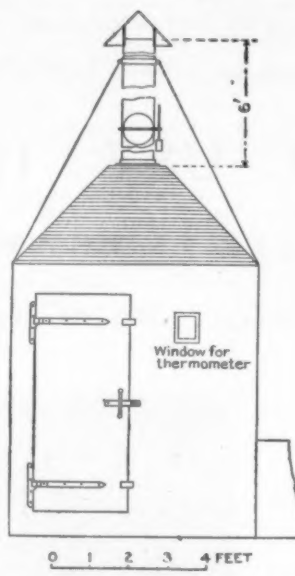


FIG. 4.—THAW HOUSE FOR FROZEN EXPLOSIVES; ELEVATION.

Dynamite is put up in sticks, which are usually dipped in paraffin to make the wrapping waterproof. As by rough handling the folded edges may be broken open and the contents of the cartridge thereby exposed to the moisture of the air, these cartridges should be handled with great care, and they are best carried to the place where they are to be used in the cartons in which they are bought.

In handling explosives the greatest care must be taken to prevent their falling or getting shocks. They must not be thrown or dropped, and portions of the powder falling from the cartridges must be carefully guarded against friction, blows, or fire.

Explosives should never be carried by railroad except in conformity with the rules of the Interstate Commerce Commission, as published by the American Railway Association. These rules make it unlawful to carry any explosives except small-arms ammunition on any public vessel or vehicle carrying passengers.

Explosives should be stored in properly placed, built, and aired magazines. Such a magazine should be far enough from other buildings or works so that if an accidental explosion occurred when the magazine was full it would do the least possible damage, and it should be placed as not to be in danger from forest, brush, or other accidental fires. Magazines are best built of brick or concrete, but they are more frequently built of wood covered with corrugated iron. In any case they should be provided with wooden floors, which should be kept free from grit and dirt. It is best that only one kind of explosive should be kept in any one magazine. If more than one kind of explosive (other than permissible explosives) must be kept in the same magazine, the magazine should be divided into rooms, by partitions, and the different explosives kept in different rooms. On no account should detonators, or blasting caps, or any device containing fulminating composition, be kept in the same magazine with any other explosive. These firing devices should be kept in a dry place by themselves.

Plans and specifications for magazines will be furnished by manufacturers of permissible explosives.

The greatest care must be taken to prevent packages of explosives from falling or getting shocks. They must not be thrown, dropped, nor rolled. Wooden boxes containing explosives should be opened with extreme care, so as to avoid friction and blows as much as possible. They should never be opened within the magazine, but in a properly sheltered place outside of the magazine and at a distance from it. They should be opened only by the use of a wooden mallet and a hardwood wedge.

The thawing of frozen explosives requires extreme care, and doing it improperly has frequently led to most serious accidents. No attempt should ever be made to thaw a frozen explosive by placing the cartridge before a fire, or near a boiler, or on steam pipes, or putting it in hot water, or by placing it in the sun. While thawing, nitroglycerin explosives are extremely sensitive and should be handled with great care. During the thawing the nitroglycerin tends to separate from the dope and run out from the cartridge (that is, to exude), and this is a source of danger.

When but a small amount of the explosive is required, it may be thawed in the thawers that are furnished by all the manufacturers of explosives and have been found safe for use as directed. The thawer consists of a water-jacketed tin vessel, in which the cartridges are placed and which is closed with a tin cover. Before the water is placed in the vessel it is warmed up to a temperature not uncomfortable to the hand put into it, and the cartridges are allowed to remain in the thawer until it is found, by gently pressing them, that they are completely thawed throughout. When thawed, the material will feel plastic, or like flour, between the fingers. When frozen, or partly frozen, the stick will feel more or less rigid and hard. It is necessary that the stick should be thawed completely, because dynamite when frozen can be detonated only with great difficulty, and any part that is frozen will be but imperfectly detonated in the hole; hence not only may such partly frozen powder fail to give its full effect as an explosive, but there is danger of a serious accident in a coal mine where such powder is used, because if a blown-out shot results the burning solid part may set fire to the dust or fire damp in the air of the mine.

Where large quantities of explosives are used daily, a small thaw house should be provided for the purpose of thawing out the frozen explosive. (See Figs. 4 and 5.) Plans and specifications, together with a bill of material for such a thaw house, will be furnished, on application, by the manufacturers of permissible explosives. The thaw house should be large enough to hold all the explosives used in one day's work. It should be heated by a small hot-water heater, placed at least 4 yards from the house, the hot water being passed into the house through iron pipes, at such a rate that the temperature in the house will not at any time be above 90 deg. F.

Thaw houses are intended only for the treatment of explosives for immediate use, and not for the storage of explosives, for if powders or dynamites are left in this high temperature and dry air for a considerable time, the moisture that is a proper part of them will be driven off, and, as stated before, this will markedly alter the character of the powder or dynamite and may lead to accidents in its use.

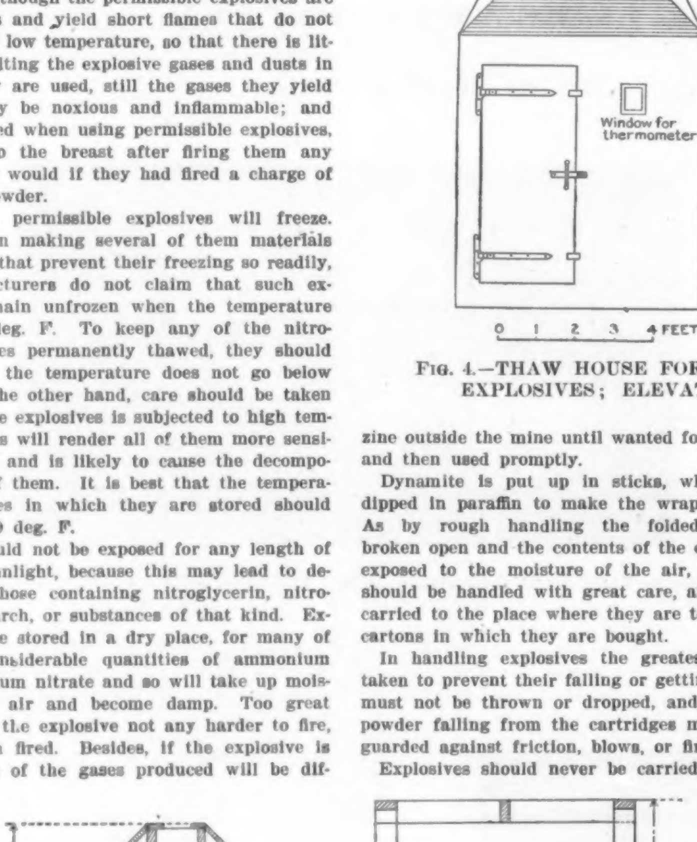


FIG. 5.—THAW HOUSE FOR FROZEN EXPLOSIVES; SECTION.

The difficulty encountered in the use of overhead conveyers running on rails is experienced especially when they have to be installed in existing workshops, in which it is difficult to find suitable points of support. Monorail tracks are not sufficiently flexible, are too heavy, and require turntables or switches operated from the ground. The "Passe-partout" system appears to avoid these inconveniences. The track consists of two parallel I beams, which are light and allow very small radii of curvature. Switching is facilitated by grouping several switching points together in a radial arrangement. The car is suspended from a rod which hangs between the beams, and oscillation is prevented by guide wheels. Shunting is effected by lightly pressing the suspending rod in the desired direction.

THE WRIGHT INJUNCTION.—II.

EXTRACTS FROM THE COURT'S OPINION AND THE BRIEFS.

Concluded from Supplement No. 1781, Page 123.

WHY CURTISS WING TIPS ARE AT DIFFERENT ANGLES.

"In order to show the court the true angular relations of the adjustable tips of defendants' machine to the wind, we present a corrected sketch marked 'Complainants' Diagram of Operation of Defendants' Machine.' Fig. 1 is the position shown by defendants' sketch, with one tip, *XC*, inclined downward 10 degrees from the normal position *XL*, and the other tip *XC'* inclined upward 10 degrees from the same line. Fig. 2 represents the machine with the motor throttled a little, in which case the main aeroplanes must be inclined a little more in order to provide support for the machine at the lower speed. The post *B* will then be inclined backward four degrees and the neutral position *XL* will be rotated in the same direction four degrees from the horizontal, and the side rudders *XC'* and *XC* will likewise be rotated four degrees from the positions shown in Fig. 1. In fact, the entire machine will be rotated together four degrees from the original position. The angle of incidence of *XC'* which was originally ten degrees, will now be 10 minus 4, or six degrees, while the angle of incidence of *XC* will be 10 plus 4, or fourteen degrees. Although both rudders are adjusted to equal angles from the normal position *XL*, there is a difference of eight degrees in their angles of incidence. Under this condition the claim of Mr. Curtiss that the angles of incidence must always be equal when the rudders are adjusted equal distances from the normal position *XL* is manifestly untrue.

"Fig. 3 represents a case where the speed of the machine is still lower and the angle of incidence greater by eight degrees than in Fig. 1, and four degrees greater than in Fig. 2. This results in an angle of incidence on *XC* of 10 plus 8, or eighteen degrees; while the angle *XC'* is 10 minus 8, or two degrees. Thus one tip will have nine times as great an angle of incidence as the other, instead of having equal angles."

WRIGHT MACHINE NOT LIKE PATENT.

Wright Brief: "It is to be understood that in the aeronautical art aeroplanes may be flat, or substantially so, or decidedly curved and be mechanical equivalents of each other in every sense of the patent in suit."

Curtiss Brief: "In the only form of the device shown in the (Wright) patent in suit each 'aeroplane' . . . is absolutely flat, and the only place where any other form is hinted at is in" (these) "lines . . . : 'These surfaces may receive more or less curvature from the resistance of the air . . .'"

"It must be kept in mind . . . that the machine of the patent is a gliding machine, and that therefore any prior gliding machine is available as prior art. In fact the patentees . . . admit . . . that the patent in suit is merely an improvement on prior machines, for they state that: 'Our invention relates to that class of flying machines in which the weight is sustained,' etc., showing that this class of heavier-than-air gliding machines was a well-known class of invention. . . . In one or two places in the specification it is specified that power might be applied to it for propulsion, but no description of how the same could be applied is given, and therefore it must be assumed that . . . it would involve no invention to apply power to any glider." Brief also alleges that certain lines in Wright patent (28-50, page 5) "admit that machines of this character provided with a forward horizontal rudder and a rear rudder were old."

It is alleged in the Curtiss brief that the only means described or hinted at for restoring equilibrium in the Wright patent is the warping of the main supporting surfaces themselves. Attention is called to wording of specification, the record of application and what patentees then stated to their attorney.

CURTISS DESCRIPTION OF HIS OWN MACHINE.

The Curtiss machine is described by Mr. Newell as follows: "It has two main supporting surfaces, both of which are curved . . . and are absolutely rigid at all times and cannot be moved, warped or distorted in any manner. The front horizontal rudder is used for steering up or down, and the rear vertical rudder is used only for steering to the right or left, in the same manner as a boat is steered by its rudder. The machine is provided at the rear with a fixed horizontal surface, which is not present in the machine of the patent, and which has a distinct advantage in the operation of defendants' machine, as will be hereafter discussed.

"Defendants' machine does not use the warping of the main supporting surfaces in restoring the lateral equilibrium, but has two comparatively small pivoted balancing surfaces or rudders. When one end of the machine is tipped up or down from the normal, these planes may be thrown in opposite directions by the operator, and so steer each end of the machine up or down to its normal level, at which time tension upon them is released and they are moved back by the pressure of the wind to their normal inoperative position.

"When defendants' balancing surfaces are moved they present equal angles of incidence to the normal rush of air and equal resistances, at each side, of the machine, and there is therefore no tendency to turn around a vertical axis as is the case of the machine of the patent, consequently no reason or necessity for turning the vertical rear rudder in defendants' machine to counteract any such turning tendency. At any rate, whatever may be the theories in regard to this matter, the fact is that the operator of defendants' machine does not at any time turn his vertical rudder to counteract any turning tendency due to the side balancing surfaces, but only uses it to steer the machine the same as a boat is steered."

"The complainants in their rebuttal affidavit have introduced an exhibit entitled 'Drawing Defendants' Machine,' which shows an exaggerated and distorted machine apparently different from that set forth in their moving affidavit."

CURTISS MACHINE NOT AN INFRINGEMENT.

"The position of the complainants is that, although

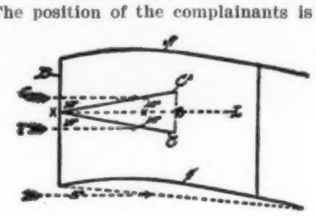


Fig. 1.

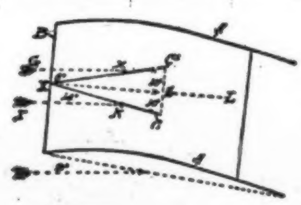


Fig. 2.

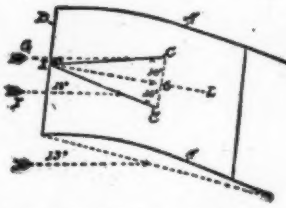


Fig. 3.

the defendants' device does not answer the wording of the claims in suit, it is an equivalent construction. Even with such broad interpretation as the complainants contend for, they substantially admit that each of the claims is limited to a construction in which the device must present different angles of incidence to the air and have a consequent turning tendency caused thereby, and that the rear vertical rudder must necessarily be used to counteract such turning tendency. Even with such a legal construction of the claims, defendants do not infringe. Defendants' machine does not answer the claims as worded, nor when construed according to the actions taken in the Patent Office and the interpretation of the claims put upon them thereby, and this, too, without taking into consideration the prior art constructions."

Mr. Newell goes on to say: "Complainants' conclusions, therefore, rest solely upon their theories as to what ought to occur in defendants' machine, and their conclusions fall to the ground unless their theories are correct. Their theories, however, have been riddled by Mr. Curtiss's affidavit in rebuttal, where he shows not only that the actual facts are that there is no appreciable difference of pressure on, or resistance offered by defendants' balancing surfaces, but that the vertical rudder is in fact not turned to counteract any turning tendency when the planes are warped. . . . The facts (are) directly denied by Mr. Herring and Mr. Curtiss (who are the ones . . . who really know about the operation of defendants' machine), but complainants' theories, even if true at any time, are shown to be founded on false or erroneous assumptions, and at most to be true only for an instant of time not sufficient in length to cause the results which they seem to think must necessarily flow from such theoretical causes. Mr. See's affidavit and that of the Wrights admit that the condition shown in Fig. 3 of the 'Curtiss Sketch,' and in Fig. 1 of 'Complainants' Diagram of Operation of Defendants' Machine,' if true, would not result in a difference of resistance or difference in angle of incidence, and they have put in Figs. 2 and 3 to try to show that the Fig. 1 condition is not the normal. This has been demolished in Mr. Curtiss's affidavit in rebuttal where it is shown that the Fig. 1 condition is the normal, and the condition shown in Figs. 2 and 3 only occurs instantaneously, and not for a sufficient length of time to cause any turning of the machine. Furthermore, even the condition of Figs. 2 and 3 does not occur when the wind changes strike the machine, for at that time, as specified by Mr. Curtiss the balancing surfaces are in their normal position and therefore meet the change of the air current at equal angles and with equal resistances, and before the balancing surface could be moved by the operator the normal condition shown in Fig. 1 reoccurs with its consequent equality of angles of incidence and consequent equality of resistances, instead of with an inequality of the same. Mr. Curtiss's affidavit also shows that the practical result in actual flight is the same whether the balancing surfaces are absolutely flat or are slightly curved.

"These results in defendants' machine are of a distinct advantage, and apparently could not take place in the machine of the patent in suit because, as explained in the Newell affidavit, the warping of the flat main supporting surfaces must of necessity result in a difference of angle of incidence, and consequent difference of resistance on the two sides. Consequently the operation of the rear vertical rudder to counteract a consequent turning tendency is unavoidable in the machine of the patent, but is not a necessity, and is not actually done in any of defendants' machines.

"The function of presenting to the atmosphere different angles of incidence and different resistances as specified in the claims in suit, obviously refers to a time when the balancing devices (whatever they may be) are actually operated for the purpose of restoring the equilibrium, and not at some time when they are not so operated. The record shows that when the equilibrium is restored in the patent in the suit, the function specified in the claims must, of necessity, occur, but they do not occur when defendants' balancing surfaces are moved.

"In short, even assuming that a legal construction of the claims as broad as the complainants contend for is allowable, the question of infringement, as substantially admitted by the complainants, depends upon theories and facts which are asserted in the moving affidavits, but equally strongly denied in the defendants' affidavits, and complainants' position depends not only on the correctness of the theories advanced by them upon the application of those theories to the actual facts. . . .

"That the word 'aeroplane' of the claims means a flat surface as shown in the patent in suit, as distinguished from a curved surface, is furthermore corroborated not only by Lieut. Selfridge, but also by the Wright brothers themselves. In the letter of Lieut. Selfridge, on pages 16 and 17 of the Wrights' moving affidavit, he states:

"'Will you kindly tell me what results you obtained on the travel of the center of pressure both on aerocurves and aeroplanes?'

"In reply of the Wright brothers to that letter, they state:

"'The travel of the center of pressure on aeroplanes is ——. The center of pressure on a curved surface is approximately ——.'

"These letters not only show that an uninterested party who was delving into this art, considered that an aeroplane meant a flat surface as distinguished from a curved surface, but the reply of the Wright brothers shows that this was what they considered themselves even as late as 1908. These facts corroborate the defendants' contention that in the patent in suit the word 'aeroplane' means a flat surface, and not a curved surface, and that the claims in suit are so limited."

There are over 400 miles of railway now in operation in Guatemala, and various extensions are in prospect. One of these contemplates the building of a line from Zacapa on the Northern Railroad, about 100 miles from the sea, to Santa Ana, on the northwestern frontier of Salvador, where it will connect with the British railway already built, and thereby with the capital of Salvador. Much of the coffee now grown in that republic will thus find an outlet to the Atlantic of which it has long been in need, and it is highly probable that the bulk of the import trade to Salvador will also be conducted along this route.

THE GREAT PARIS FLOOD.

THE CAUSE OF THE GREAT INUNDATION.

Continued from Front Page.

to swell the Seine. In the region around Paris the difference in elevation of the ground is slight. The river bed at the Tuileries is only a few feet higher than at Asnières after the Seine has taken a wide sweep around the whole city. The flooding of the sewers was due to the fact that the outlet of the main sewer at Clichy is flush with the river. It was placed there contrary to the plan of the engineer who devised the modern scheme of sewers. The blame for the inundation of the underground roads has not been definitely settled as yet; it is probably due to thoughtlessness and disregard of the Seine's possibilities on the part of all concerned.

The immediate remedies called for are clearly the building of higher and stronger embankments for the river, and changes in the outlets of the sewer system that will prevent the floods from backing in. The condition, however, is a permanent one, and nature must be guarded against more effectively. In the eighteenth century it was proposed to fill in the ground of Paris to a height above all possible floods. That magnificent scheme is entirely out of the question to-day. Another plan suggested is to build a channel from the Seine above Paris to the first loop below the city and turn the flood water into this. That would cost 100,000,000 francs and would relieve only the city and its western suburbs.

The Times engineer seems to prefer the plan of damming up the streams of the upper affluents of the Seine, retaining the water in time of flood and letting it out when the rivers are low. He shows that the present freshet cannot be attributed to deforestation, for tree planting has been going on for years in the Seine watershed. He believes the floods to be due almost entirely to the nature of the soil, which cannot be changed. Meanwhile Paris, having enjoyed its fortnight of excitement, is on the lookout, as it ever is, for new sensations.—The New York Sun.

Discussing the question of the "Water Supply for the Lock Canal at Panama," in a paper read before a recent meeting of the American Society of Civil Engineers, Mr. Julio F. Sorzano sought to prove that during the three or four months' dry season on the Isthmus the various leakages and necessary uses of the canal and its structures would so reduce the water level of the lake at Gatun that it would be impossible to maintain in the canal the 40-foot depth which the present design prescribes. The various causes of loss of water were given as follows: Surface evaporation, border evaporation, percolation through the bottom and sides of the reservoir, lockage water, normal waste contingent to lockage, water for hydro-electric plants, structure leakage and waste, accident losses, and, finally, a general grouping of miscellaneous losses, such as the needs of the local population and industries, the uses of passing vessels, etc. A computation was given proving these leakages and uses would so reduce the water that the supply would be insufficient.

DEPUTIES' UNDIGNIFIED APPROACH TO THE CHAMBER.

By courtesy of Illustrated London News.

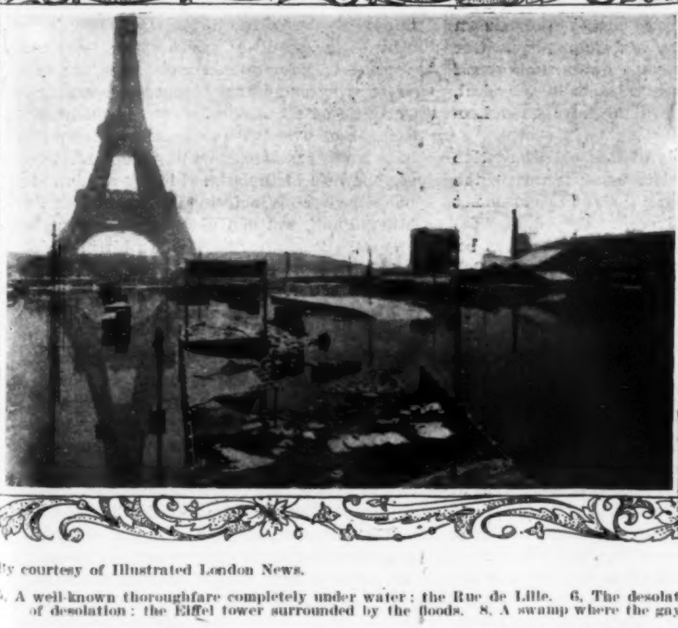
1, The water almost over the Pont Solferino: the Seine at its greatest height. 2, A submerged railway station: all that could be seen of the Gare des Invalides. 3, Trying to keep dry the cellars of a famous restaurant: water being pumped out of the restaurant Ledoyen. 4, At the Palais Bourbon during the flood: a temporary pathway.

TYPICAL SCENES IN THE FLOODED SECTION.

THE GREAT PARIS FLOOD.

THE FLOOD AT ITS HIGHEST ON SATURDAY, JANUARY 29TH; THE ILE DE LA CITÉ AND THE SWOLLEN SEINE.

This drawing was made from a point on the Quai des Augustins, opposite the Ile de la Cité, on which stands the Cathedral of Notre Dame. The Quai des Augustins is the "Booksellers' Row" of Paris, for there are situated the stalls of the second-hand booksellers, the favorite haunt of the book lover in quest of finds in the shape of rare editions. The boxes full of books that are shown clamped to the parapets of the quays are apparently safe, the parapet being here above water, but the booksellers have had to stop business long ago. The height of the water can be judged by the fact that the barges are moored at the extreme edge of the quay, or lower wharf, many feet below the parapet. This explains why they are some way out in the river. The first bridge seen in the picture is the Pont St. Michel. On the left, further on, is Notre Dame, and, further still, on the right bank, is the Morgue. The top of a great crane jib in mid-stream may also be noted. To the left of Notre Dame may be seen part of the Palais de Justice, and just to the right of Notre Dame, in the distance, the tower of the Gare de Lyon, and, further to the right, the chimneys of Bercy.



By courtesy of Illustrated London News.

5. A well-known thoroughfare completely under water: the Rue de Lille. 6. The desolate appearance of a usually crowded street: the Avenue Ledru-Rollin. 7. Guardian of a scene of desolation: the Eiffel tower surrounded by the floods. 8. A swamp where the gayest crowd is usually to be found: the inundated Champs Elysees.

TYPICAL SCENES IN THE FLOODED SECTION.

THE GREAT PARIS FLOOD.

LEONARDO DA VINCI.—II.

THE ENGINEER AND MACHINIST.

BY EDWARD P BUFFET.

Concluded from Supplement No. 1781, Page 128.

LEONARDO'S CREED—THEORY AND PRACTICE.

Read a few more proverbs of science from Leonardo's notes and observe how intensely Baconian he is, both as to the useful object and the experimental method; yet how he preserves a balance between concrete and abstract processes:

"Every instrument requires to be made by experience."

"It is necessary to begin by experience and, thanks to her, to discover the reason of the phenomena."

"Instrumental or mechanical science is of all the noblest and most useful, seeing that by means of this all animated bodies that have motion perform all their actions; and these movements are based on center of gravity, which is placed in the middle, dividing unequal weights, and it has dearth and wealth of muscles and also lever and counter-lever."

"Experience never errs; it is only your judgments that err by promising themselves effects such as are not caused by your experiments."

"Experience does not err; only your judgments err by expecting from her what is not in her power. Men wrongly complain of experience; with great abuse they accuse her of leading them astray, but they set experience aside, turning from her with complaints as to our ignorance causing us to be carried away by vain and foolish desires to promise ourselves, in her name, things that are not in her power; saying that she is fallacious. Men are unjust in complaining of innocent experience, constantly accusing her of false evidence."

"The man who blames the certainty of mathematics feeds on confusion and never can silence the contradictions of sophistical sciences which lead to a sophistical quackery."

"There is no certainty in sciences where the mathematical sciences cannot be applied, or which are not in relation to these mathematics."

"Science is the captain and practice the soldiers."

"Those who fall in love with practice without science are like a pilot who enters a ship without a rudder or a compass and who never can be certain whither he is going."

"Be assured that it is necessary to study with system and patience the nature of the objects that one wishes to reproduce; otherwise he loses his time and very uselessly prolongs the work."

With what a shock to an age which still revered Bernard and Aquinas must have fallen da Vinci's "Treatise on Painting," containing such a doctrine as this:

"No human investigation may be treated as pure science if it does not pass through mathematical demonstrations. When you say that those sciences which begin and end in cogitation have also their truth, I deny it on good grounds. I will not admit that in such reasonings experience is of no account, since without experience there cannot be a bit of certainty."

"It is said that mechanical knowledge," remarked Leonardo also, "is that engendered by experience, and the scientific that which is born in the spirit, while the semi-mechanical is born in science and ends in a manual operation. But it seems to me that those sciences are vain and full of error which are not born of experience, mother of all certitude, and which do not lead back to verification in experience; that is to say, whose origin, means and end do not pass through any of the five senses."

UNIVERSAL PERCEPTION.

Those who are best qualified to know Leonardo have divined that the supreme condition of his liberty was solitude. Though far from being an unsocial man, there was in him a necessity for isolation of mind. He expiated his privileges with loneliness.

While his genius was "universal" in the practical sense of a complete horizon, it was such too in the philosophical sense of grasping "universals," or even in the mystical sense of entering into close communion with the universe. Something akin to that state of mind, so elusive of definition, which mystics profess to experience, seems to dominate a few sane, observant, and creative minds who are at the furthest possible remove from mysticism in the results they produce.

The Medicean age, when he lived, was one of introspection; a Swedenborg or a Pascal would have been more at home in it than a Goethe or a Newton. Leonardo's output, however, is of a sort that indicates little self-consciousness, but rather a broad interest in the things and people around him.

May we not affirm that in his hours of brooding he was gazing, not at himself, but through himself at the Macrocosm? That in this way he enjoyed a scope of vision and power of generalization which even his omnivorous curiosity could not have reached by its convivial exercise? That his nurtured intuitions were more prolific than mere study could have been? That, in plain words, the best understanding of men and things is not always gained in constant companionship with them?

Amiel, a man of subjective bent, very unlike da Vinci, has described his own fits of abstraction in words which may furnish a clue to Leonardo's mind:

"I am a spectator, so to speak, of the molecular whirlwind which you call individual life. I am conscious of an incessant metamorphosis, an irresistible movement of existence, which is going on within me, and this phenomenology of myself serves as a window opened upon the mystery of the world."

The generalizing skill in portraiture, which Leonardo shared with others of his artistic school has been verified by a mechanical test of recent times. With "composite photography" the faces of several persons may be superposed and blended; if they belong to a special class the resultant emphasis of feature distinguishing them from groups otherwise composed may be taken as characteristic of the class. This device has proven the accuracy of Renaissance painters in depicting typical physiognomy.

But, if it be true that genius reaches profound conclusions "intuitively," this does not mean that the mind really ignores details or reasons loosely, but rather that the hard work is done in the subconsciousness. Intellectual bricks cannot be made without straw, and great results can come only from a great brain. Astounding was the precision of Leonardo's mind and eye. When dividing the human figure in his measurements, he counted "degrees," "points," "minutes," "minimes," and "semi-minimes," these forming a duodecimal series the smallest being a 248,832nd.

UNITY.

Stimulating to the modern engineer as may be the unlimited scope of Leonardo's genius, a rarer inspiration is derived from its unity. Other men have been versatile, but they have kept their talents in separate compartments of the mind. They have turned from one avocation to another for change and contrast. The poet has cooled his brain cells by chair caning or poultry farming, while the fashioner of brass and iron into useful shapes has flown from the monotony of his work to the strains of flute and violin. Yet, for the ordinary man his several activities are in nowise related—no more than for the savage are the sea currents which bear his boat with the moon which lights his way.

For Leonardo all the laws of truth, whether in science, art, or industry, were consonant expressions of the same omniscient nature. On those drudging members which other men esteem less honorable, he bestowed more abundant honor, and this he did, not by segregation, but by association. There was no forced elevation of mechanics at the expense of the fine arts, but a free social equality between them, with mutual help and dependence. Power and utility were shown to be blood relatives of taste and humanity. Our present-day practician who cannot quite rid himself of a sense of squalor in his mental, as in his physical, surroundings, should learn his dignity from old Leonardo.

The reason that the example of this old Florentine is almost as fresh and needful to us as it was to the men of his own day consists herein: That Leonardo not merely believed in unity, but lived it.

A theoretical acceptance of nature's oneness has become commonplace. This was not so always. Our Aryan progenitors supposed that the different manifestations of the physical universe were impowered by independent deities. The sun, the sky, the darkness, the dawn, the rain, the lightning, were actuated to regular or capricious changes by separate in-dwelling spirits. As the human mind has progressed it has tended to fuse these divisions of control into a single power. One of the most fascinating studies in the history of thought is to follow the paths along which, from exceedingly remote times, pluralism, as a conception of nature, has been yielding to monism.

To our minds it has become a truism that all things which move are inflexibly geared together. For Leonardo, before the time of Paracelsus, to perceive the same, was an honorable feat, but had he done no more than that he would scarcely be required for our emulation. What distinguishes him as a model unique for all time is his realization of his scientific faith; he was its vital exponent.

Science was to him the schoolmaster of industry, and mechanics the preceptor of art. Painting was applied anatomy, and anatomy was explained by the laws of the lever. As an ornithologist he wrote a treatise on the flight of birds, which was a kinematical study, and on this he based the principles of the airship. If the flying machine ever be perfected, it will follow, as he foresaw, the pattern of winged animals, just as the fleet vessel must approximate in contour to the fish and every optical instrument must simulate the eye.

Optics was one of the sciences that Leonardo studied profoundly and applied in his pictures. Perspective he well understood; his notebooks reveal how he scrutinized the human countenance from judiciously chosen viewpoints. Binocular vision he found intelligible and employed to advantage. His figures are artfully caused to stand out in space by sharp delineation of the important features, toward which the observer's eyes will instinctively converge, while a soft obscurity envelops those lines which naturally will be seen out of focus.

For Leonardo neither esthetic nor industrial products were at an end exclusively, but he co-ordinated them as objects of intrinsic worth and called upon the methods of both to lend reciprocal assistance. To the gears and levers of his mechanical sketch books he deemed it not ignoble to devote those pencil strokes which might have outlined a Madonna or an Adonis. Botanical studies aided him in adroit imitation of foliage. The crests of breaking waters, whose powers he measured, were molded by his fancy into grotesque shapes which his pencil congealed.

"Especially where there is relation and proportion," wrote Leonardo, "is there a place for calculation; and the proportion is found not only in numbers and measure, but also in sounds, weights, times, and places, and in every force, whatsoever it be."

"Mathematics," said he, "are necessary to the painter for the painting forms a part of them."

"Painting is a new creation of nature; it embraces all the forms of the visible. . . . Perspective is the brake, the rudder, of the painter."

"The science of mechanics is as universal as that of painting, for every action is produced by motion and motion is the cause of all life."

"There is no sound without motion," he remarks, "and no motion without force. The force is caused by the motion in the weight. The blow is born of the weight and the motion."

A certain breadth of functions was common among da Vinci's contemporaries. Michael Angelo and Raphael were architects as well as artists. Responsibility for the effect of a painting did not end with putting on the pigments. I translate from Paul Errera, in the Revue de l'Université de Bruxelles:

"With the picture the artist composes the frame; with the fresco the entire decoration of the chapel or hall; marble, wood, stucco, wrought iron, all come out of the same shop. The saying is found in Vasari *à propos* of Verrocchio: 'Herein is the supreme harmony of the beautiful works of the Renaissance—they are conceived for a determined position; they fit their surroundings, and their proportions are calculated, as are those of the architecture; to which they are connected, moreover, with a thousand perceptible bonds.'"

INCIDENTS OF DA VINCI'S LIFE.

Leonardo's birthplace, which gave his surname, was the village of Vinci, in Florentine territory. His father, Piero, son of Guido, was a lawyer in a line of lawyers. Soon after Leonardo's birth, which occurred in 1452, his peasant mother, Caterina, married a man of her own rank in life, but Pietro brought up the boy in his house with the advantages that his fast-growing means afforded.

In the studio of Andrea del Verrocchio, at Florence, da Vinci spent his youth, learning to draw and paint, to model and chisel, to found and weave. With the brush he soon became so surpassingly proficient as to inspire a legend that he drove his master to abandon the profession in chagrin.

Anatomy of the body taught him what to draw, while anatomy of the eye led him into subtle confidences with the observer whom he wished to impress. A pre-Raphaelite in very truth, he ignored tradition and learned from nature wisdom in portrayal. When his fancy passed from the beautiful to the grotesque, his scientific spirit led him to produce a heraldic dragon synthetically from reptiles and insects.

While developing his powers, Leonardo did not neglect the amenities of life. Vivacity and magnetism of temperament, generosity of disposition, were joined to a physique of combined strength and beauty which dazzled his contemporaries. It is handed down that he could twist horseshoes in his fingers and ride unbroken horses. Such a glow he left in the world after four centuries Robert Louis Stevenson could write: "Of the misbegotten changelings who call themselves men, and prate intolerably over dinner tables, I never saw one who seemed worthy to inspire love—no, nor read of any, except Leonardo da Vinci, and perhaps Goethe in his youth." No picture of Leonardo in his young manhood remains, and how he then looked we can but dimly infer from the hirsute portraits of his mature years.

The age of Lorenzo de Medici was dawning upon Florence, and after Leonardo's debut as an artist *sui juris* he is found working under patronage of that magnificent prince. About 1480 lies an obscure period in his annals, and it is supposed by some that during this epoch he made certain mysterious travels in the East—to Egypt, Constantinople, Asia Minor, Armenia. It is believed that he served as engineer for the sultan of "Babylon," to wit, Cairo. His mind had early turned to mechanical pursuits and the financial unsuccess of his paintings disposed him to military architecture as more lucrative. We can only conjecture how much added wisdom he brought back from the Orient to Italy.

The prime of his career began about 1484, with the service of Duke Ludovico Sforza il Moro, at Milan, to whom he was recommended by Lorenzo. It is told that he was called to Ludovico's court as a violinist, and that he bore a curious silver lyre of his own invention. Those who thus relate do not overestimate his proficiency in the creation and theory of sound, and the incident may be founded on fact, but the main grounds for his employment by Ludovico were his ability as a military engineer and his skill as a sculptor. Among ten qualifications confidently named by him to the duke, military engineering is prominent, although civil engineering, painting and sculpture are not omitted. At this time he bid for work which subsequently, in a procrastinating manner, he carried out—the huge bronze equestrian statue of Ludovico's father, Francesco Sforza. The fortunes of war soon gave this ponderous image to destruction.

At Milan, da Vinci's activities and studies were multifold. Intermittently he built war engines, planned court pageants, researched in optics, geometry, and anatomy, and engaged in vocations too varied to describe. He improved the Martesana canals, thereby enriching the city, and he benefited the agriculture of Licino by irrigation. In this period he painted the "Last Supper," which shares pictorial supremacy of the world with Raphael's "Sistine Madonna" and Michael Angelo's "Last Judgment." Before the end of the ten years during which he labored by fitful intervals on this mural masterpiece, the monks whose dining room it was to adorn grew tired enough of waiting for its completion, since Leonardo's art was long in realization of his conceptual types, lax in application, and infinitely painstaking in detail. The remark has been made of him that he finished nothing, or if he finished, it was only to commence anew. Unhappily, this painting furnishes an instance where his alliance of art and science was fickle, for, anxious to obtain the unusual effect of oils in fresco, or to employ a method that allowed more deliberation and revision than the ordinary mural processes, he taxed his chemical skill and victimized the priceless picture in an unsuccessful experiment. After repeated peelings and restorations, it is doubtful that any of the work now visible is his.

In this long term of residence under Ludovico's protection, Leonardo became principal of an academy which grew famous and which has been called the first scientific and experimental academy in Italy. He accumulated no fortune at Milan, for he was not a sharper, and throughout most of his life a bad money maker, his powers of mind and hand being appreciated by his patrons in any way except pecuniarily.

This luxurious if not lucrative period of Leonardo's life was brought to an end by the army of Louis XII. of France, which entering Lombardy in 1499, soon reduced Milan and made Ludovico a prisoner. Leonardo took refuge in Venice and Florence, at which latter place he engaged in painting and other professional work. He projected at this time the canalization of the Arno and proposed to move the St. John baptistry to a new site by means of water power from the river, neither of which plans he was permitted to execute.

Some two years after the fall of Milan he placed his services at the disposal of Cesare Borgia, who was under French patronage and a bitter enemy of Ludovico. For this famous assassin da Vinci built military machines, strengthened fortifications and traveled, minutely mapping wide regions of Tuscany. He was thus commissioned private architect and general engineer of the duchy of Urbino.

His facile changes of his political clothes will be condemned especially by those who esteem "patriotism" above all virtues. The circumstances, however, were peculiar. Artists and savants were a privileged class of supernumeraries; then, too, the partisan life in Italy was mixed and shifty. But best he may be vindicated through drawing due distinction between dynastic pretensions and public interests. In the downfall of a state its proletariat have usually cause for rejoicing.

Less consistent with an immaculate character, was Leonardo's readiness to hire himself out to make gun carriages, catapults and battering rams. To be sure he followed the almost universal judgment of mankind that in preparing for war business is business. But Leonardo must be held to stricter account than ordinary men. His clear mind should have been revolted by the incongruity of his fighting machines with the sacred themes of those paintings to which he devoted himself. His imagination could indeed be touched with the "bestial frenzy" of war as a motive for a great picture. In his private dealings he was forbearant, victimized rather than victimizing. It is odd that he assumed the canons of morality binding on individuals to be annuled at the caprice of a prince or will of a nation. Like his earlier compatroit, the Saint of Assisi, he might be never so stern with himself, yet deferred obediently to external powers.

Relieved by Borgia's downfall, in 1503 from his active labors, Leonardo settled for three years at Florence, which city he might instead have been called to lay in ruins, had not the duke's quietus frustrated the plan for an attack. He there, in cartoon competition booked an order to print a battle scene for the Hall of Council. This cartoon it was, which depicted the combat of the Anghiari, the "bestial frenzy," as he called it, of war, and it may be that the choice of the subject was tinged with remorse. Again, however, science betrayed art, and failure of a process by which he had hoped to bake the picture into the wall occasioned his abandonment of the contract. Instability was one of his besetting faults. But at Florence he painted the Mona Lisa del Gioconda, for which subsequently Francis I. of France paid 45,000 francs and in which biographers have sought to find the one romance of Leonardo's solitary life. He there conducted rigorously scientific studies of the flight of birds, their feathery and muscular structure and the resistance of the air, applying the results of these researches to designs of machines for artificial flight.

To Milan he came once more in 1506, and dwelt for several years in peace and preferment under Charles of Amboise, who was appointed governor by the French king, Louis XII. That monarch, a visitor at Milan, became an admiring patron of Leonardo, who was endowed with the dignity of his painter and engineer. Da Vinci became an honored guest and habitual dweller at the villa of Francesco Melzi, a young disciple who afterward followed him like a dog. In this term of residence at Milan he engaged in hydraulic work, including the Martesana canals again, and the basin of San Christoforo. Two other pursuits were expeditions to the mountains in the north of Lombardy to study their formation and researches in the dissecting laboratory which perfected his skill as an anatomist.

At the close of 1511, Milan was shaken by another revolution, the French party being driven out and Maximilian Sforza, son of Ludovico, coming in. Two years later Leonardo turned his face toward Rome, where Giovanni da Medici, son of Lorenzo, was Pope. The successor of St. Peter was pleased at first with da Vinci's curious scientific experiments, more so than with those which later he undertook in concocting a new unctuous medium for a picture that he had been commissioned to paint. But suspicions of necromancy or heresy were aroused against him and even heeded by the Pope. His anatomical dissections were deemed especially an offense. On the whole, he received little encouragement at Rome, therefore he turned from his brushes to birds and airships and ere long quitted the city. It has been suggested that he was injured through Michael Angelo's jealousy. This young contemporary shared a little of Leonardo's versatility and worked as architect on St. Peter's church. The same is true of Raphael, whose artistic production was done by hatching the eggs that Leonardo had laid. With all due respect for the junior members of this triad, it seems to be a descending scale.

Rome's atmosphere of chill for Leonardo could have caused him few shivers; he was fortunate that it caused him no burns. He cared little for the canons of the hierarchy and his note books bristle with caustic epigrams on the monks of that vicious age. While he lived and died a nominal Catholic, and got along better than Galileo, his spirit was that of the critical Erasmus though hardly that of the austere Savonarola. Leonardo was indeed an inveterate free thinker, though not in the sense of being an irreligious man, as is evidenced both by his sympathetic treatment of sacred themes for painting and by his inscriptions, as a man of science, to the prime mover of the world, creator of power and order.

Yet once more Milan was entered by the French, whose king, Francis I. then come to the throne, added to the favor of his predecessor toward Leonardo an adoring enthusiasm. By his invitation the artist left Italy for France at the close of 1515, followed by Francesco Melzi, and took up a residence at Chateaux Cloix, in Touraine.

There he lived, doing a little painting and canal work, until he dropped away. This occurred in 1519, his sixty-seventh year, when age had palsied his hand but had not sapped his intellect.

"I do not believe," said the king, "that there is in the world another man as wise as Leonardo, not only as sculptor, painter and architect, but as a profound philosopher." This verdict may stand until the next superman appears.

It has been alleged that Leonardo wasted his talents by diffusion of effort and that his actual accomplishments were comparatively slight. Therein is a half-truth. His paintings were few compared with Raphael's, and most of his inventions were still-born. Had he specialized as men specialize to-day, he would have achieved something so spectacular as to be the world's transcendent marvel. But for conviction that his definite results were tremendous, it is needful only to examine the records which he left behind. Rarely were his discoveries followed up, yet that does not belie that he made them.

LEONARDO'S REMAINS.

Of Leonardo's literary output, the work that has longest been published, though not in his lifetime, is his "Treatise on Painting." He projected various others, such as a "Book of Motion," "Treatise on Impact," "Machine Elements," "Book of Gravity," and "Book of Momentum." These, if completed, are said to be lost, but no doubt many of the ideas contained in them have been preserved elsewhere in his teeming and bewilderingly arranged manuscripts.

Data of his long productive years were kept by him in the form of notes and drawings covering thousands of sheets. A single page would contain, say, a bar-bending machine ensemble, details of ratchets and worm gears, the design of an architectural cupola, the study for a grotesque head, or a philosophical sentiment—any of these subjects dovetailing into the odd corners left by others on the paper, as they did in his mind. It was his purpose to classify his manuscripts, but his death left them unassorted.

By testament they passed with his other chattels to his famulus, Francesco Melzi, who brought them back to Italy and cherished them for 50 years. After Melzi's death some of them fell into the hands of one Mazzenta, who, being convinced that he had no right to them, restored them to Dr. Orazio Melzi, a gift which the latter requited by bestowal on him of many manuscripts. But Pompei Leoni, sculptor and brass founder to Philip II., of Spain, was then living in Milan, where he had accumulated an art collection. He induced Melzi to bid from Mazzenta the volumes before rejected; in this way Leoni obtained seven of them and otherwise got hold of three more. These manuscript books were taken apart for him and rebound in a great volume, 18 x 27 inches, with an arrangement, or lack of arrangement, attributable to the convenience of the binder. More than 1,700 groups of drawings were comprised in 402 folios, windows being left in the leaves for matter on the back sides of the sheets. By reason of its magnitude or atlas-like character, this collection became known as the "Codice Atlantico."

This Codex was carried by its owner to Madrid but brought back to Milan and lay in the Ambrosian library until 1796. In that year it, with twelve or fourteen other volumes of Leonardo's manuscripts, was stolen by Bonaparte and sent to Paris as loot. After Waterloo it was returned to its Italian home, where now it rests. The minor manuscripts, however, had been deposited in the Institute, at Paris, instead of the National Library with the Codice Atlantico, and by an oversight they were not restored to Italy then nor have they been since. Within the last 20 years they have been published under the editorship of Ravaisson-Molien, in facsimile, accompanied by the deciphered text in both Italian and French, thus filling six huge folio volumes.

Still more lately the Accademia dei Lincei, at Rome, with governmental aid, has completed a limited edition of the Codice Atlantico in facsimile plates mounted on large sheets like drawing paper. The process of reproduction is superior to that used for the French collection, but there is no transliterated text. This is more of a drawback for the casual researcher than might be supposed, and unless he has learned the trick of Leonardo's chirography, he will find it as incomprehensible as Arabic. The characters are small, detached letters, fairly clear, but at first stubbornly defying interpretation. The difficulty is lessened, if not entirely removed, by information that Leonardo, who was left-handed, habitually wrote backward, so that the manuscript must be read as in a looking-glass.

Other collections of da Vinci's sketches and writings

are preserved in European libraries, including those of Florence and Venice, of Windsor and the British Museum. After long obscurity, much of them has been published. Among them is a little notebook on the Flight of Birds.

But no other equals in importance or interest the Codice Atlantico. This work may now be seen at the New York Public Library and probably at other large ones in the United States. Rarely does anybody ask to look at it; but either artist or engineer who fails to spend at least a day in its perusal is contemning the most majestic documentary monument of his profession.

Huge facsimile editions like those that have been described are manifestly, however, unavailable for general use, and a digested classification of the matter contained is imperative for any but the most leisurely reader. No illustrated popularization of the scientific material in either the Institute collection or the Codice Atlantico has, to my knowledge, hitherto been brought out in the English language. This has been done in France by M. A. Ronna, who, in the Bulletin de la Société d'Encouragement pour l'Industrie Nationale has described da Vinci's hydraulic machinery from the Atlantico, with a long sketch of his life; and in Germany by Prof. Beck, who has treated his mechanical output quite exhaustively. The former has been helpful in preparing the present article; to the latter, special credit may be reserved for a future paper in which we shall examine some of Leonardo's work.—American Machinist.

PROGRESS IN WIRELESS TELEGRAPHY.—I.*

A NOBEL PRIZE LECTURE.

BY GUGLIELMO MARCONI.

THE discoveries connected with the propagation of electric waves over long distances, and the practical applications of telegraphy through space, which have gained for me the supreme honor of sharing the Nobel prize for Physics, have been to a great extent the result of one another.

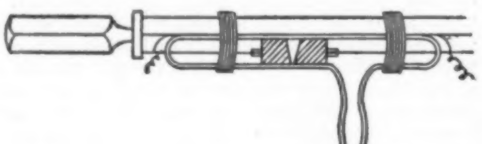


FIG. 1.

The application of electric waves to the purposes of wireless telegraphic communication between distant parts of the earth, and the experiments which I have been fortunate enough to be able to carry out on a larger scale than is attainable in ordinary laboratories, have made it possible to investigate phenomena and note results often novel and unexpected. In my opinion many facts connected with the transmission of electric waves over great distances still await a satisfactory explanation, and I hope to be able in this lecture to refer to some observations which appear to require the attention of physicists.

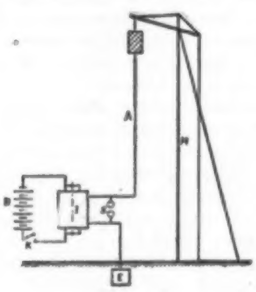


FIG. 2.

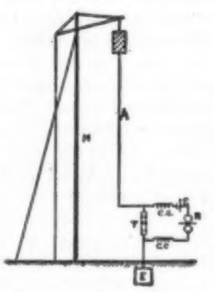


FIG. 3.

In sketching the history of my association with radiotelegraphy, I might mention that I never studied physics or electrotechnics in the regular manner, although as a boy I was deeply interested in these subjects. I did, however, attend one course of lectures on Physics under the late Prof. Rosa, at Livorno, and I was, I think I might say, fairly well acquainted with the publications of that time dealing with scientific subjects, including the works of Hertz, Branly and Righi. At my home near Bologna, in Italy, I commenced early in 1895 to carry out tests and experiments with the object of determining whether it would be possible by means of Hertzian waves to transmit to a distance telegraphic signs and symbols without the aid of connecting wires. After a few preliminary experiments with Hertzian waves I became very soon convinced that if these waves or similar waves could be reliably transmitted and received over considerable distances a new system of communication would become available possessing enormous advantages over flashlights and optical methods, which are so much dependent for their success on the clearness of the atmosphere. My first tests were carried out with an ordinary Hertz oscillator and a Branly coherer as detector, but I soon found out that the Branly coherer was far too erratic and unreliable for practical work.

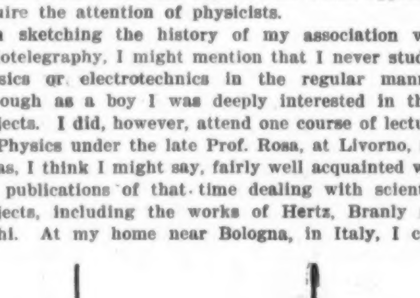


FIG. 4

After some experiments I found that a coherer, constructed as shown in Fig. 1, and consisting of nickel and silver fillings placed in a small gap between two silver plugs in a tube, was remarkably sensitive and reliable. This improvement, together with the inclusion of the coherer in a circuit tuned to the wavelength of the transmitted radiation, allowed me gradually to extend up to about a mile the distance at which I could affect the receiver.

Another, now well-known, arrangement which I adopted was to place the coherer in a circuit containing a voltaic cell and a sensitive telegraph relay actuating another circuit, which worked a tapper or trembler and a recording instrument. By means of a Morse telegraphic key placed in one of the circuits of the oscillator or transmitter, it was possible to emit long or short successions of electric waves, which would affect the receiver at a distance and accurately reproduce the telegraphic signs transmitted through space by the oscillator. With such apparatus I was able to telegraph up to a distance of about half a mile. Some further improvements were obtained by using reflectors with both the transmitters and receivers, the transmitter being in this case a Righi oscillator. This arrangement made it possible to send signals in one definite direction, but was inoperative if hills or any large obstacle happened to intervene between the transmitter and receiver.

FIG. 5.

In August, 1895, I discovered a new arrangement which not only greatly increased the distance over which I could communicate, but also seemed to make the transmission independent from the effects of intervening obstacles. This arrangement consisted in connecting one terminal of the Hertzian oscillator, or spark producer, to earth, and the other terminal to a wire or capacity area placed at a height above the ground, and in also connecting at the receiving end one terminal of the coherer to earth and the other to an elevated conductor.—Figs. 2 and 3. I then began to examine the relation between the distance at which the transmitter could affect the receiver, and the elevation of the capacity areas above the earth, and I very soon definitely ascertained that the higher the wires or capacity areas the greater the distance over which it was possible to telegraph.

Thus I found that when using cubes of tin about 30

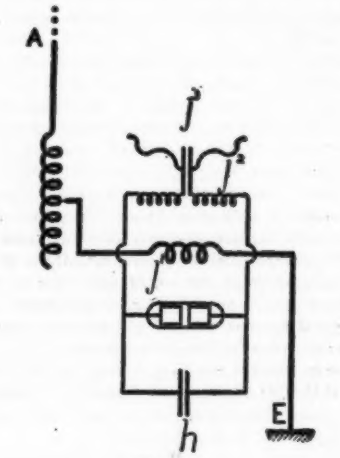


FIG. 6.

cms. side as elevated conductors or capacities, placed at the top of poles 2 meters high, I could receive signals at 30 meters distance, and when placed on poles 4 meters high at 100 meters, and at 8 meters high at 400

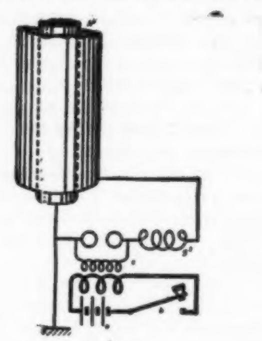


FIG. 7.

meters. With larger cubes 100 cms. side, fixed at a height of 8 meters, signals could be transmitted 2,400 meters all around.[1] These experiments were continued in England, where, in September, 1896, a distance of 1¾ miles was obtained in tests carried out for the British

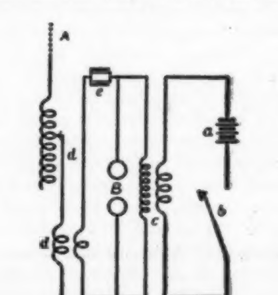


FIG. 8.

government at Salisbury. The distance of communication was extended to four miles in March, 1907, and in May of the same year to nine miles. Tape messages obtained during these tests, signed by the British government officers who were present, are exhibited. In all these experiments a very small amount of elec-

* Address delivered in Stockholm by Mr. Marconi on the occasion of the awarding of his share of the Nobel prize.

[1] See "Journal" of the Institution of Electrical Engineers London, 1899. Vol. XXVIII., page 278.

trical power was used, the high-tension current being produced by an ordinary Rhumkorff coil. The results obtained attracted a good deal of public attention at the time, such distances of communication being considered remarkable.

As I have explained, the main feature in my system consisted in the use of elevated capacity areas, or vertical wires, attached to one pole of the high frequency oscillators and receivers, the other pole of which was earthed. The practical value of this innovation was not understood by many physicists for quite a considerable period, and the results which I obtained were by many erroneously considered simply due to efficiency in details of construction of the receiver, and to the employment of a large amount of energy. Others did not overlook the fact that a radical change had been introduced by making these elevated capacities and the earth form part of the high-frequency oscillators and receivers. Prof. Ascoli, of Rome, gave a very interesting theory of the mode of operation of my transmitters and receivers in the Elettricista (Rome) issue of August, 1897, in which he correctly attributed the results obtained by the use of elevated wires or antennæ. Prof. A. Slaby, of Charlottenburg, after witnessing my tests in England in 1897, came to somewhat similar conclusions.[2]

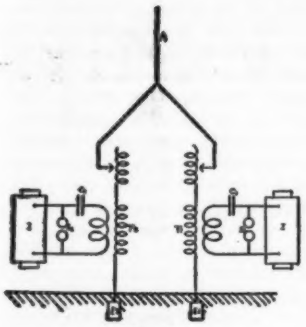


FIG. 9.

Many technical writers have stated that an elevated capacity at the top of the vertical wire is unnecessary. This is true if the length or height of the wire is made sufficiently great, but as this height may be much smaller for a given distance if a capacity area is used, it is more economical to use such capacities, which now usually consist of a number of wires spreading out from the top of the vertical conductor.

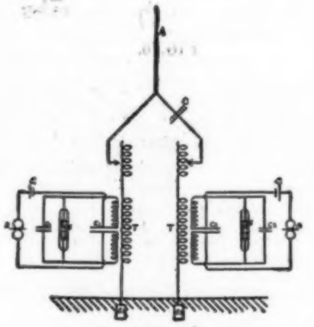


FIG. 10.

The necessity or utility of the earth connection has been sometimes questioned, but in my opinion no practical system of wireless telegraphy exists where the instruments are not connected to earth. By "connecting to earth" I do not necessarily mean an ordinary metallic connection as used for ordinary wire telegraphs. The earth wire may have a condenser in series with it, or it may be connected to what is really equivalent, a capacity area placed close to the surface of the ground—Fig. 4. It is now perfectly well known that a condenser, if large enough, does not prevent the passage of high-frequency oscillations, and, therefore, in these cases the earth is for all practical purposes connected to the antennæ. After numerous tests and demonstrations in Italy and in England over distances varying up to 40 miles, communication was established for the first time across the English Channel between England and France in March, 1899.[3] (Fig. 5.)

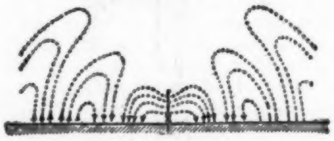


FIG. 11.

[2] See A. Slaby, "Die Funkentelegraphie," Berlin, 1897, Verlag von Bronhard Simion; also A. Slaby, "The New Telegraphy," the Century Magazine, April, 1898, Vol. LV., page 867.

[3] See Journal of the Institution of Electrical Engineers, 1899, London, Vol. XXVIII., p. 291.

From the beginning of 1898 I had practically abandoned the system of connection shown in Fig. 2, and instead of joining the coherer or detector directly to the aerial and earth, I connected it between the ends of the secondary of a suitable oscillation transformer containing a condenser and tuned to the period of the electrical waves received. The primary of this oscillation transformer was connected to the elevated wire and to earth—Fig. 6. This arrangement allowed of a certain degree of syntony, as by varying the period of oscillation of the transmitting antennæ it was possible to send messages to a tuned receiver without interfering with others differently syntonized.[4]

FIG. 12.

As it is now well known, a transmitter consisting of a vertical wire discharging through a spark gap is not a persistent oscillator—the radiation it produces is strongly damped. Its electrical capacity is comparatively so small and its capability of radiating energy so large that its oscillations decrease or die off with great rapidity. In this case receivers or resonators of a considerably different period or pitch are likely to be affected by it. Early in 1899 I was able to improve the resonance effects obtainable by increasing the capacity of the elevated wires by placing adjacently to them earthed conductors, and inserting in series with the aerials suitable inductance coils.[5] By these means the energy storing capacity of the aerial was increased, whilst its capability to radiate was decreased, with the result that the energy set in motion by the discharge formed a train or succession of feebly damped oscillations. A modification of this arrangement, by which excellent results were obtained, is shown in Fig. 7.

In 1900 I constructed and patented transmitters which consisted of the usual kind of elevated capacity area and earth connection, but these were inductively coupled to an oscillation circuit containing a condenser, an inductance and a spark gap, the conditions which I found essential for efficiency being that the periods of electrical oscillation of the elevated wire or conductor should be in time or resonance with that of the condenser circuit—Fig. 8[6]. The circuits, consisting of the oscillating circuit and radiating circuit, were more or less closely "coupled" by varying the distance between them. By the adjustment of the inductance inserted in the elevated conductor and by the variation of the capacity of the condenser circuit the two circuits were brought into resonance, a condition which, as I have said, I found essential in order to obtain efficient radiation.

Part of my work regarding the utilization of condenser circuits in association with the radiating antennæ was carried out simultaneously to that of Prof. Braun, without, however, either of us knowing at the time anything of the contemporary work of the other.

A syntonic receiver has already been shown in Fig 6, and consists also of a vertical conductor or aerial, connected to earth through the primary of an oscillation transformer, the secondary circuit of which included a condenser and a detector, it being necessary that the circuit containing the aerial and the circuit containing the detector should be in electrical resonance with each other, and also in tune with the periodicity of the electric waves transmitted from the sending station. It is also possible to couple to one sending conductor several differently tuned transmitters, and to a receiving wire a number of corresponding receivers, as is shown in Figs. 9 and 10, each individual receiver responding only to the radiations of the transmitter with which it is in resonance.

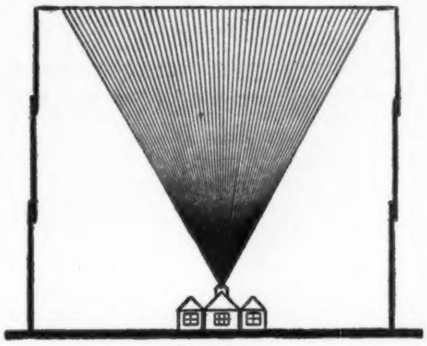


FIG. 13.

At the time (eleven years ago) when communication was first established by means of radiotelegraphy between England and France, much discussion and speculation took place as to whether or not wireless telegraphy would be practicable for much longer distances than those then covered, and a somewhat general opinion prevailed that the curvature of the earth would be an insurmountable obstacle to long-distance transmission, in the same way as it was, and is, an obstacle to signalling over considerable distances by means of light flashes. Difficulties were also anticipated as to the possibility of being able to control the large amount of energy which it appeared would be necessary to cover long distances. What often happens in pioneer work repeated itself in the case of radiotelegraphy—the anticipated obstacles or difficulties were either purely imaginary or else easily surmountable, but in their place unexpected barriers manifested themselves, and recent work has been mainly directed to the solution of problems presented by difficulties which were certainly neither expected nor anticipated when long distances were first attempted.

CERTIFIED TRACK CHART OF S.S. PHILADELPHIA. AMERICAN LINE.
SHEWING POINTS WHERE MR. G. MARCONI RECEIVED MESSAGES FROM CORNWALL, ENGLAND

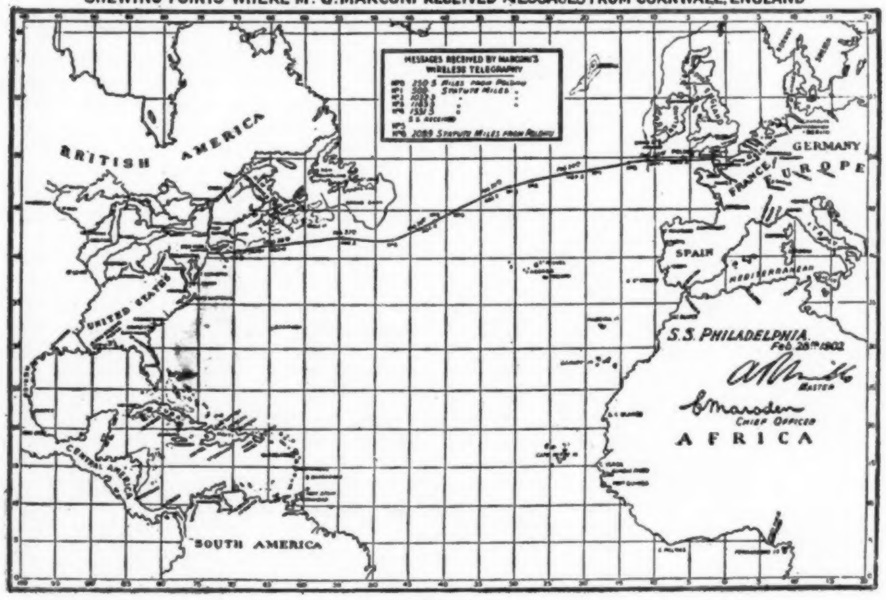


FIG. 14.

With regard to the presumed obstacle of the curvature of the earth, I am of opinion that those who anticipated difficulties in consequence of the shape of our planet had not taken sufficient account of the particular effect of the earth connection to both transmitter and receiver, which earth connection introduced effects of conduction which were generally at that

[4] British Patent No. 12326 of June 1st, 1898; also No. 6982 of April 1st, 1899.

[5] See "Etat Actuel et Progres de la Telegraphie sans Fil" by A. Blondel and G. Ferrie, read at the Congress International d'Electricite, Paris, 1900; also "Journal" of the Society of Arts, 1901, Vol. XLIX, page 509.

[6] See British Patent No. 7777 of 26th of April, 1900; also "Journal" of the Society of Arts, Vol. XLIX., May 17th, 1901, page 510-511.

time overlooked. Physicists seemed to consider for a long time that wireless telegraphy was solely dependent on the effect of free Hertzian radiation through space, and it was years before the probable effect of the conductivity of the earth between the stations was satisfactorily considered or discussed.

Lord Rayleigh, in referring to transatlantic telegraphy, stated, in May, 1903: "The remarkable success of Marconi in signalling across the Atlantic suggests a more decided bending or diffraction of the waves round the protuberant earth than had been expected, and it imparts a great interest to the theoretical problem."[7]

Prof. J. A. Fleming, in his book on "The Principles of Electric Wave Telegraphy,"[8] gives diagrams showing what is now believed to be the diagrammatic representation of the detachment of semi-loops of electric strain from a simple vertical wire—Fig. 11. As will be seen, these waves do not propagate in the same manner as free radiation from a classical Hertzian oscillator, but glide along the surface of the earth. Prof. Fleming further states in the above-quoted work: "The view we here take is that the ends of the semi-loops of electric force, which terminate perpendicularly on the earth, cannot move along unless there are movements of electrons in the earth corresponding to the wave-motions above it. From the point of view of the electronic theory of electricity, every line of electric force in the ether must be either a closed line or its ends must terminate on electrons of opposite sign. If the end of a line of strain abuts on the earth and moves, there must be atom-to-atom exchange of electrons, or movements of electrons in it. We have many reasons for concluding that the substances we call conductors are those in which free movements of electrons can take place. Hence the movements of the semi-loops of electric force outwards from an earthed oscillator of Marconi Aerial is hindered by bad conductivity on the surface of the earth and facilitated over the surface of a fairly good electrolyte, such as sea water."

Prof. Zenneck[9] has carefully examined the effect of earthed transmitting and receiving aerials, and has endeavored to show mathematically that when the lines of electrical force, constituting a wave front, pass along a surface of low specific inductive capacity, such as the earth, they become inclined forward, their lower ends being retarded by the resistance of the conductor to which they are attached. It, therefore, seems well established that wireless telegraphy, as practiced at the present day, is dependent for its conductivity of the earth, and that the difference in operation over long distances on the conductivity between the surface of the sea and land is sufficient to explain the increased distance obtainable with the same amount of energy in communicating over sea as compared to over land.

I carried out some tests between a shore station and a ship at Poole, in England, in 1902, for the purpose of obtaining some data on this point, and I noticed that at equal distances a perceptible diminution in the energy of the received waves always occurred when the ship was in such a position as to allow a low spit of sand about 1 kilometer broad to intervene between it and the land station. I, therefore, believe that there was some foundation for the statement so often criticised, which I made in my first English patent of June 2d, 1896, to the effect that when transmitting through the earth or water I connected one end of the transmitter and one end of the receiver to earth.

In January, 1901, some successful experiments were carried out between two points on the South Coast of England, 186 miles apart, i. e., St. Catherine's Point, Isle of Wight, and the Lizard, in Cornwall.[10]—Fig. 12. The total height of these stations above sea level did not exceed 100 m. whereas to clear the curvature of earth a height of more than 1,660 m. at each end would have been necessary. The results obtained from these tests, which at the time constituted a record distance, seemed to indicate that electric waves produced in the manner I had adopted would most probably be able to make their way round the curvature of the earth, and that, therefore, even at greater distances, such as those dividing America from Europe, the factor of the earth's curvature would not constitute an insurmountable barrier to the extension of telegraphy through space.

The belief that the curvature of the earth would not stop the propagation of the waves, and the success obtained by syntonic methods in preventing mutual interference, led me in 1900 to decide to attempt the experiment of testing, whether or not it would be possible to detect electric waves over a distance of 4,000 kilometers, which, if successful, would immediately prove the possibility of telegraphing without wires between Europe and America. The experiment was, in my opinion, of great importance from a scientific point of view, and I was convinced that the discovery of the possibility to transmit electric waves across the Atlantic Ocean, and the exact knowledge of the real conditions under which telegraphy over such distances could be carried out, would do much to improve our understanding of the phenomena connected with wireless transmission. The transmitter erected at Poldhu, on the coast of Cornwall, was similar in principle to the one I have already referred to, but on a very much larger scale than anything previously attempted.[11] The power of the generating plant was about 25 kilowatts.

Numerous difficulties were encountered in producing and controlling for the first time electrical oscillations of such power. In much of the work I obtained valuable assistance from Prof. J. A. Fleming, Mr. R. N. Vyvyan, and Mr. W. S. Entwistle. My previous tests had convinced me that when endeavoring to extend the distance of communication, it was merely sufficient to augment the power of the electrical energy of the sender, but that it was also necessary to increase the area or height of the transmitting and receiving elevated conductors. As it would have been too expensive to employ vertical wires of great height, I decided to increase their number and capacity, which seemed likely to make possible that efficient utilization of large amounts of energy. The arrangement of transmitting antennæ which was used at Poldhu is shown in Fig. 13, and consisted of a fan-like arrangement of wires supported by an insulated stay between masts only 48 meters high and 60 meters apart. These wires converged at the lower end, and were connected to the transmitting apparatus in a building.

For the purpose of the test, a powerful station had been erected at Cape Cod, near New York, but the completion of the arrangements at that station were delayed in consequence of a storm, which destroyed the masts and antennæs. I, therefore, decided to try the experiments by means of a temporary receiving station erected in Newfoundland, to which country I proceeded with two assistants about the end of November, 1901.

The tests were commenced early in December, 1901, and on the 12th of that month the signals transmitted from England were clearly and distinctly received at the temporary station at St. John's, in Newfound and. Confirmatory tests were carried out in February, 1902, between Poldhu and a receiving station on the steamship "Philadelphia," of the American Line. On board this ship readable messages were received by means of a recording instrument up to a distance of 1,551 miles, and test letters as far as 2,099 miles from Poldhu—Fig. 14.

The tape records obtained on the "Philadelphia" at the various distances were exceedingly clear and distinct, as can be seen by the specimens exhibited.

These results, although achieved with imperfect apparatus, were sufficient to convince me and my coworkers that by means of permanent stations and the employment of sufficient power it would be possible to transmit messages across the Atlantic Ocean in the same way as they were sent over much shorter distances. The tests could not be continued in Newfoundland owing to the hostility of a cable company, which claimed all rights for telegraphy, whether wireless or otherwise, in that colony.

(To be continued.)

[7] See "Proceedings" of the Royal Society, Vol. 72, May 28th, 1903.

[8] See page 348 (published by Longmans, Green and Co., London. 1906).

[9] See J. Zenneck. "Annalen der Physik.," 23, 5, p. 846, September, 1908. "Physikal Zeitschrift," No. 2, p. 50; No. 17, p. 553.

[10] See "Journal" of the Society of Arts, London, Vol. XLIX., page 512, 1901.

[11] Royal Institution of Great Britain, Lecture by G. Marconi, June 13th, 1902.

ELECTRIC VALVES.

Now that the use of higher voltages for bulk supply is becoming more general, the question of protection of electrical plant against damage due to resonance surges in underground lines and atmospheric disturbances as well as in overhead lines—which for commercial reasons are likely to become more common in the near future—is one that electrical engineers should turn their attention to at the present time.

Where trouble has already occurred in central stations it has generally been put down to faulty design or bad insulation of the machines. There is no doubt, however, that the damage is caused very often by the setting up—owing to a short circuit or the sudden action of the automatic fuse—of a serious rise of potential, which is stored in the windings of the alternator, and can only flow off through the capacity of the transformer or by perforating the insulation. Such voltages rapidly deteriorate the insulation of sunk windings, and so it becomes necessary to find some means of overcoming this difficulty. Two such pieces of apparatus are now available, and may be used in conjunction with each other or separately, according to the conditions controlling the line, area of distribution, capacity of plant, etc., and are known as the "electric valve" and "Moscicki condenser."

The electric valve consists of a number of spark-gaps arranged as follows: The first spark-gap is placed in series with a sufficiently high resistance, so as to avoid high-frequency oscillations, and the remaining spark-gaps are indirectly connected to earth through small condensers, the last spark electrode being connected direct to earth. The spark-gaps are formed between the edges of sharp round disks of non-arcing metal insulated from each other and from the earth connection. The capacity required is obtained by these disks and a central rod which is connected to earth, and also acts as a support for the disks which are insulated from it. The resistance in series with these disks is a metallic one, and obviates the trouble usually due to high resistances of graphite or carborundum.

The first spark-gap is adjustable, and is inclosed in a glass cylinder. Six or more sets of spark-gaps are connected in parallel—each through a high-tension fuse—to a common disk, which acts as one pole, while the cast-iron base to which the columns are bolted acts as the other pole. The columns are protected from dust and damage by a glass cylinder, which rests on rubber pads on the cast-iron base, and is protected on top by an insulated cover.

The characteristics of the electric valve may be summed up as follows: (1) absolute prevention of high-frequency currents; (2) unlimited capacity for dealing with any energy; (3) the adjustable spark-gaps being inclosed in glass cylinders, there is no likelihood of dust getting between the knobs and causing premature action of the apparatus; (4) the automatic extinction of the arc; (5) erection or dismounting very rapid

The Moscicki condenser resembles an extremely long Leyden jar, with the difference that the neck of the jar—where the coatings end—is considerably thickened. The coatings are produced by a chemical silvering process, and a heavy deposit on both the inside and outside of the jar is obtained, which is further strengthened and protected by a copper deposit. The jaws are then mounted in a tin or brass tube, on the top of which a high-tension insulator is arranged, and carries the contact connected to the inner coating. The outer coating is connected to the metal tube, and the intermediate space is filled with a mixture of glycerin and water. It is then hermetically sealed, and consequently the condenser can be used in any position. Glass is used for the dielectric, owing to its great dielectric capacity and uniformity.

The usual type of condenser as used for line protection consists of a number of tubes, as described above, mounted on a wrought-iron frame, and the inner coatings are connected in parallel through high-tension fuses to a common terminal, to which the line is connected. The outer coatings are connected to the tin or brass tubes, and connected to earth by means of the framework, which is so arranged that each tube can be easily replaced or removed when it is necessary.—Nature.

JEWELERS' WAX.

To MAKE jewelers' wax, take common rosin and heat it in a vessel until it flows freely; then add plaster of Paris, stirring continually while adding the powder. Care should be taken not to make the mixture too stiff. When it appears of the proper consistency, pour some of it on a slate or marble slab and allow it to cool; then insert the point of a knife under the flattened cake thus formed, and try to pry it off. If it springs off with a slight metallic ring, the proportions are right; if it is gummy and ductile, there is too much rosin; if it is too brittle and crumbles, this indicates that there is too much plaster.

This is what is sold for jewelers' cement. It is used for filling gold-headed canes, umbrella handles, cementing stones in ring settings, and also for holding very thin pieces of metal on a faceplate for drilling, cutting disks, or turning off the surface. I gave this formula to a friend who had some very artistic tile for a fireplace, and after having set as many as he could one evening, he forgot that the wax only needed rewarming in order to use it the next day, so in order to keep it fresh he poured water in the iron pot; in the morning the wax had become insoluble, owing to the action of the water on the plaster. So it is advisable not to wet the wax until it is put to its final use and place.—Machinery.

A safety lamp for detecting small percentages of firedamp without lowering the flame of the lamp, has been invented by two English engineers. The inventors recently gave a demonstration with the new lamp, showing how the introduction of sodium carbonate into the flame—by means of a piece of asbestos saturated with it—converted the blue cap, produced by gas, into a bright yellow flame, without lowering the lamp flame.

HALLEY'S COMETARY STUDIES.

HIS OWN ACCOUNT OF HIS INVESTIGATIONS ON ORBITS.

HALLEY'S treatise, which bore the title "Astronomiæ Cometicæ Synopsis," (i. e. "A Synopsis of the Astronomy of Comets") was presented to the Royal Society in 1705, and was published in 1706 in volume 24 of the Society's Transactions, page 1882-1899. The following account of this work is taken from Volume IV. of Baddam's abridgment of the Memoirs of the Royal Society (London, 1739). The original spelling and punctuation are retained here.

The ancient Egyptians and Chaldeans (if we may credit Diodorus Siculus) being furnished with a long series of observations, could predict the rising, or appearing of comets; but since, they also are said by the same arts to have foretold earthquakes and tempests, it is past all doubt, that their knowledge in these matters was rather the result of astrological calculations than of astronomical theories of the motions of the celestial bodies; and the Greeks, who were the conquerors of both these nations, scarcely found any other sort of learning among them than this; so that it is to the Greeks themselves, as the inventors, especially to the great Hipparchus, that we owe this astronomy, which is now so greatly improved; but Aristotle's opinion (viz., that comets were nothing else than sublunary vapors or airy meteors) prevailed so far amongst the Greeks, that this sublimest part of astronomy lay altogether neglected; since none could think it worth while to observe, and give an account of the wandering and uncertain paths of vapours floating in the Æther; whence it is, that we have nothing certain handed down from the ancients concerning the motion of comets; but Seneca the philosopher, considering the Phænomena of two remarkable comets of his time, made no scruple to place them amongst the celestial bodies, taking them to be stars of equal duration with the world itself; tho' he owns, that their motions were regulated by laws not then discovered; at length, he foretells (which has proved no vain prediction) that time and diligence would unfold these mysteries to some future ages, who would be surpris'd how the ancients could be so ignorant of them, after that some lucky interpreter of nature would have pointed out in what parts of the heavens comets wandered, and shewn what, and how great they were; yet almost all astronomers differed from Seneca in this; and Seneca himself has not left any account of the Phænomena of the motion, whereby he might support his hypothesis, nor assigned the time of their appearing, which might enable posterity to determine anything in this matter: So that after Mr. Halley had turned over several histories of comets, he could find nothing at all, that could give any assistance herein, before A. D. 1337, when Nicephorus Gregoras, an historian and astronomer of Constantinople, had pretty accurately described the path of a comet amongst the fixed stars; but he too loosely assigns the time, so that this undermined comet only deserved to be inserted in the catalogue, on account of its having appeared almost 400 years ago; the next comet A. D. 1472, which moved the swiftest of all, and came nearest to the earth was observed by Regiomontanus; this comet (so frightful on account both of the magnitude of its body and its tail) in the space of a day moved 40 deg. of a great circle in the heavens, and it is the very first of which, any proper observations have been handed down to us; for all those who considered comets before Tycho Brahe, that great restorer of astronomy, supposed them to be below the moon, and so took but little notice of them, imagining them to be no other than vapours: But in the year 1577 Tycho Brahe applying himself seriously to the study of astronomy, and having procured large instruments for making celestial mensurations, with greater exactness and certainty than the ancients could ever hope for; there appeared a pretty remarkable comet, to the observation of which Tycho vigorously applied himself, and he found by several unquestionable trials, that it had no sensible diurnal parallax; and consequently, that it was not only no aerial vapour, but much higher than the moon; nay, and might be reckoned amongst the planets for anything that appeared to the contrary, notwithstanding the cavilling opposition of some schoolmen; to Tycho succeeded the sagacious Kepler, who having the advantages of Tycho's observations, found out the true and physical system of the world, and vastly improved astronomy; for he demonstrated, that all planets revolved in planes passing through the centre of the sun, and describing elliptical curves, observing this law, that the area's of the elliptic sectors taken at the centre of the sun, in the Focus of the ellipsis, are always proportional to the times, wherein the corresponding arches are described; he also discovered, that the distances of the planets from the sun are in the sesquilateral ratio of their periodical times, or that the cubes of the distances are as the squares of the times; this great astronomer had the opportunity of observing two comets, one of which was very remarkable; and from his observations of these, he concluded, from several indications of an annual parallax, that comets move freely thro' planetary orbits, with a motion not much different from a rectilinear one, but which he could not determine: Next Hevelius, a noble emulator of Tycho, following Kepler's steps, embraced the same hypothesis of the rectilinear motion of comets, he himself having very accurately observed several of them; yet he complained, that his calculations did not altogether agree with the appearances in the heavens, and he was aware that the path of comets was incurvated toward the sun: At length, that extraordinary comet of 1680 descended from a vast distance, and as it were, in a perpendicular line toward the sun, and ascended from him again with an equal velocity; this comet appearing constantly for four months, by the peculiar and remarkaable curvity of its orbit, seemed above all others the most adapted for investigating the theory of their motion; and the Royal observatories at Paris and Greenwich being founded some time before, and committed to the care of the most famous astronomers, the apparent motion of this comet was (as far as human sagacity could reach) very accurately observed by M. Cassini and Mr. Flamstead: Not long after, that incomparable geometrician Sir Isaac Newton, not only demonstrated that what Kepler had found, did necessarily obtain in the planetary system, but likewise that all the Phænomena of comets plainly follow from the same principles, which he fully illustrated by the above-mentioned comet of 1680; and at the same time shewed the way of geometrically constructing the orbits of comets, and to the surprise of all men solved a problem whose intricacy rendered it worthy of so great a genius; and he proves that this comet revolved round the sun in a parabolic orb in such a manner that the areas estimated at the centre of the sun were proportional to the times: Mr. Halley pursuing the steps of so great a man attempted (and he presumes not without success) to bring the same method to an arithmetical calculation; for having collected together all the observations of comets he obtained the following table, the result of almost immense calculation.

* Journal of the Royal Astronomical Society of Canada.

THE ASTRONOMICAL ELEMENT OF THE MOTIONS, IN A PARABOLIC ORBIT, OF ALL THE COMETS HITHERTO OBTAINED.

Passage of Perihelion, London Time.				Longitude of				Inclination of Orbit.		Distance from Sun at Perihelion.
				Perihelion.		Asc. Node.				
	d.	h.	m.	°	′	°	′	°	′	
1337, June	2	6	25	37	59	84	21	32	11	0.40666
1472, February	28	22	23	45	34	281	46	5	20	0.54273
*1531, August	24	21	18	301	39	49	25	17	56	0.56700
1532, October	19	22	12	111	7	80	27	32	36	0.50910
1556, April	11	21	23	278	50	175	42	32	6	0.46390
1577, October	26	18	45	129	32	25	52	74	33	0.18342
1580, November	28	15	00	109	6	18	57	64	40	0.59628
1585, September	27	19	20	8	51	37	42	6	4	1.09358
1590, January	29	3	45	216	54	225	31	29	41	0.57661
1596, July	31	19	55	228	15	3.2	12	55	12	0.5.293
*1607, October	16	3	50	302	16*	50	21	17	2	0.58680
1618, October	29	12	23	2	14	76	1	37	34	0.37975
1652, November	2	15	40	28	19	88	10	79	28	0.84750
1661, January	16	23	41	115	59	82	30	32	36	0.44851
1664, November	24	11	52	130	41	81	14	21	18	1.02576
1665, April	14	5	16	71	54	228	2	76	5	0.10649
1672, February	20	8	37	47	0	297	30	83	22	0.69739
1677, April	25	00	38	137	37	236	49	79	3	0.28059
1680, December	8	00	6	262	40	272	2	60	56	0.00612
*1682, September	4	7	39	302	53	51	16	17	56	0.58328
1683, July	3	2	50	85	30	173	23	83	11	0.56020
1684, May	29	10	16	238	52	268	15	65	49	0.96015
1686, September	6	14	33	77	0	350	35	31	22	0.32500
1698, October	8	16	57	270	51	267	44	11	46	0.69129

Those marked with a star (*) are successive apparitions of Halley's Comet.

[Then follows a general table to compute the Motion of Comets in a parabolic Orbit, together with an explanation of its construction and use.]

It is to be observed that the five first comets, the third and fourth of which was seen by Petrus Apianus, and the fifth by Paulus Fabricus, as was the tenth by Mestlinus in the year 1596, have not the same degree of certainty with the rest, the observations not having been made with the proper instruments or the necessary exactness, and therefore, disagreeing with each other, they can by no means be reconciled with a regular calculation; Blanchini alone observed at Rome the comet Anno 1684; and the astronomers at Paris the last comet in 1698, whose path they have described in an unusual manner: this very obscure comet, tho' swift and pretty near the earth, escaped our observations: Mr. Halley forbore to insert into his catalogue the two remarkable comets that appeared, the one in November 1689, and the other in February 1702, for want of observations; for directing their course towards the southern part of the world, and being scarcely visible in Europe, they were not observed by persons equal to the task: It is to be observed that 11 of the comets in Mr. Halley's catalogue moved direct, i. e., according to the order of the signs; viz. those in the years 1532, 1556, 1580, 1585, 1618, 1652, 1661, 1672, 1680, 1684 and 1686; and that the other 13 were retrograde, i. e., moved contrary to the order of the signs.

Upon weighing all these things, and comparing the rest of the elements of the motions of these comets with each other, it will appear, that their orbits are disposed in no certain order; and that they are not confined like the planets to the zodiac, but that they move indifferently, every way both retrograde and direct; whence it is plain, that they are not moved by Vortices; the distances of this Perihelia are found to be sometimes greater and sometimes less; whence we have reason to suspect, that there are a great many more comets, which being at remote distances from the sun, and being obscure and without a tail, may for that reason escape our observation.

We have hitherto considered the orbits of comets as perfectly parabolic, from which supposition it would follow, that comets, being impelled by a centripetal force toward the sun, do descend from infinite distances, and by their fall acquire so great a velocity, as to convey them into the remotest spaces of the system, and by a perpetual Nisus tending upward, never afterward to return again to the sun; seeing then that the appearing of comets is very frequent, and that none of them is found to move in an hyperbola, or with a greater velocity than it would acquire in falling toward the sun, it is more credible, that they revolve about the sun in very excentric orbits, and return after very long periods; for, thus their number is definite and perhaps not so very great; and the spaces between the sun and the fixed stars are so immense, that there is room for a comet to perform its period, however large it may be: for the Latus rectum of an ellipsis is to the Latus rectum of a parabola, having the same Perihelian distance, as the Aphelian distance in the ellipsis is to its whole axis; but the velocities are in the sub-duplicate ratio of the same: wherefore, in very excentric orbits, this ratio approaches very nearly to a ratio of equality, and the small difference which arises on account of the greater velocity in a parabola, is very easily compensated in determining the situation of the orbit; therefore, the principal use of the elements of the motions in this table is, that whenever a new comet appears, we may by comparing the elements, know whether it is one of those that formerly appeared; and consequently, we may determine its period, and the axis of its orbit, and foretell its return; and Mr. Halley tells us that he had several reasons to induce him to believe, that the comet in 1531, which was observed by Apian, was the same with that described in 1607 by Kepler and Longomontanus, and which he himself had seen and observed upon its return again in 1682; all the elements agree, and there is no other difference than the inequality of their periods; which yet is not so considerable, as that it may not be ascribed to physical causes; for Saturn's motion is disturbed in such a manner by the other planets, especially Jupiter, that its periodical time is for some whole days uncertain; how much more may a comet be subject to such irregularities, whose orbit rises almost four times higher than Saturn's, and whose velocity, tho' never so little augmented, may change its orbit from an ellipsis to a parabola; that it was the same comet, is farther confirmed, from that observed, in the summer of 1456, to pass retrograde, almost in the same manner, between the sun and the earth; which tho' it was not observed astronomically by any, yet Mr. Halley conjectures, that was the same with the former, from its period and the manner of its transit; whence he ventures to foretell its return in 1758, and if this happens, there will be no further cause to doubt, but that the rest may likewise return; astronomers will therefore have a large field to exercise themselves in for several ages, before they can determine the number of so many and so great bodies that revolve round the common centre of the sun, and reduce their motions to certain rules: Mr. Halley was apt to believe, that the comet of 1532 was the same as that observed by Hevelius in the beginning of 1661; but Apian's observations, which are the only ones we have, are too inaccurate, to determine anything certain from them in so nice an affair: Sir Isaac Newton delivers a method of constructing the orbits of comets by three

accurate observations, Philos. Natural. Princip. Mathemat. lib. III. which afterwards Dr. Gregory fully and clearly illustrated in the fifth book of his physical and geometrical astronomy.

Here one thing is to be observed, viz., that some of these comets have their nodes so near the annual orbit of the earth, that should it happen at the time of the return of a comet, that the earth was near its node, whilst the comet passes with an incredible velocity, it would also have a very sensible parallax, and which would be to the sun's parallax in a given ratio; whence, upon such like transits, there would be a very favorable opportunity (which yet seldom happens) of determining the distance of the sun from the earth; which hitherto could be concluded but very loosely, and that only by means of the parallax of Mars in opposition to the sun, or that of Venus in the Perigæum: and tho' indeed it is thrice greater than the parallax of the sun, yet it is scarcely perceptible with any instrument, and this use of comets was suggested by the famous geometrician Nio Facia; for the comet of the year 1472 had a parallax 20 times greater than that of the sun; and had the comet Anno 1618 arrived, about the middle of March at its descending node, or had the Comet Anno 1864 come a little sooner to its ascending node, being very near the earth they would have had still more sensible parallaxes; of all the comets there were none that approached nearer the earth than Anno 1680; for, upon a calculation, it was no further distant towards the north from the annual orbit, than the sun's semidiameter (or the radius of the moon's orbit, as Mr. Halley suppose) and that too in November 10th 1 hr. 6 min. P. M.; at which time it had been in conjunction with the earth as to longitude, there might have been observed in its motion a parallax equal to that of the moon: Mr. Halley leaves it to philosophers to discuss what consequences would arise from the appulse, contact or collision of the celestial bodies, which yet is not altogether impossible.

THE CHINESE CALENDAR.

To the Editor of the SCIENTIFIC AMERICAN:

IN my article on the Chinese calendar in the preceding issue of SCIENTIFIC AMERICAN SUPPLEMENT, pages 114 and 115, I did not refer to the fact that the sexagenary cycle was used for days as well as for years by China. That is to say, the days are named and numbered in continuous groups of sixty, and as this method does not synchronize with the days of the month, or moon, it leads to a curious mixture of dates that often puzzle historians. For example, the current native almanacs from China show the first day of the first month quite correctly, but just below these dates the reader finds the stem and branch characters which indicate the sexagenary day. It may be anywhere from Kiah-Tsu to Kwei-Hai (1st to 60th day). It so happens that the current Chinese year began on the day known as Ping-Woo, or 43d day. It, therefore, follows that the 18th day of the month will be the 60th day. Then the next day, the 19th of the month, will be day first. This makes the first Chinese month end on Kiah-Suh or the 11th day of the next cycle of 60 days. In other words, this first Chinese month, which begins with the first day and ends with the 29th in the monthly order, also begins (strange as it may sound) on the 43d day and ends on the 11th day.

As Japan used this same system until a comparatively recent time, many Japanese calendar clocks in our museums and private collections contain this 60-day feature. As many owners of these artistic timepieces do not know what the characters mean, it was thought best to bring this point out clearly. Both China and Japan now use our clocks and watches, and Japan has adopted our Gregorian calendar. But China adheres to her ancient method of counting her days.

Just one more little point. Near the end of the article I speak of a "sidereal lunation." Please read this sidereal month. This is a period of time between 27 and 28 days, and ancient records show that a lunar zodiac of these numbers of lunar mansions or domiciles were used, but the later figure was favored because it was capable of being quartered into periods of 7 days. It is, therefore, my firm opinion that this fact fixed our 7-day week and that these 7 days came under the influence of, or were later named after the sun, moon, and five of the planets, thus pushing the origin of the Jewish Sabbath and the Christian Sunday very far back. Modern skygazers who have had the chance to see the moon and stars in the clear atmosphere of the East can realize how natural it was that these early calendar constructors should mark off the moon's zone of action into a stellar dial of 28, sometimes by single stars equally spaced, at other times by groups of stars not so well placed. This slightly arbitrary, but even number (28) being divisible by the much-used 4, which was more or less connected with the four cardinal points of the compass, the four corners of the world, the four seasons, and so on, made the creation of the 7-day period a most logical and natural step. DANIEL ARTHUR.

New York, N. Y.

SCIENCE NOTES

A bulletin of the University of California announces the discovery of further proof of the identity of heliotropism in animals and plants. The new results were obtained by Professors Loeb and S. S. Maxwell. Since fish and Daphnia are too large to allow very exact determinations of the relative heliotropic effect of the different parts of a spectrum, experiments were also made with a smaller form of animals, namely, the newly hatched nauplii of Balanus perforatus, which they were able to obtain in unlimited quantities. These animals possess the most intense positive heliotropism of any form thus far found. The frequency curve for the distribution of these animals in the spectrum gave the maximal density of the gathering in the freen, the highest ordinate being possible toward the yellowish green. Experiments proved that the heliotropic reactions of swimming animals are identical with those of swimming algæ.

Dr. Franz Fischer has discovered that when air is heated under certain conditions there is formed ozone. If we heat air to a very high temperature and then cool it quickly down to the normal, this sudden cooling has the effect of producing ozone. A Vienna electric firm has constructed an ozone-producing fan on this principle, using a Nernst radiator which gives a heat of 2,000 deg. C. The fan brings a current of air over the incandescent substance which is fixed, and the air becomes heated and is then cooled by mixing with the surrounding air. Ozone is thus formed which is sent out by the air fan. The apparatus consists of a small flywheel and air fan combined which is run by an electric motor; the fan draws in the air and sends it by a funnel-shaped vessel into the part containing the Nernst heater. The whole apparatus mounted on a base does not weigh more than 30 pounds. About 4 per cent of the oxygen is ozonized, and this amount cannot be exceeded on the present principle, so that there is nothing to fear from a too great amount of ozone. One such device will suffice to purify the air of a large hall, and the amount of power used is very low.

In an article published in the Astrophysical Journal Mr. W. H. Mitchell says that many of the characteristic features of the sun-spot spectrum appear to be due to the absorption of various oxides and hydrides existing as vapors in the spot regions. As most of these are regarded as low-temperature products, it is further supposed that water-vapor may also exist on account of the reduced temperature. Unfortunately many of the recorded observations in the region 5,860-6,000 are widely discordant so far as their identification with water-vapor lines is concerned, and it is possible that they may really be faint solar lines of other elements so close to the water-vapor positions as to be mistaken for them unless very great dispersion is employed. Determinations made with a powerful spectroscope at Haverford indicate that the supposed widening of the water-vapor lines in the spot-spectrum band may be a subjective effect caused by the dark umbral background. Hale has noted that the water-vapor lines show no evidences of circular polarisation in the sun-spot spectrum, suggesting that it is probable that they are unaffected by sun-spot absorption. From this it would appear that up to the present we have no definite evidence of the presence of water-vapor in sun-spots.

The London Times reports that a novel method of killing moths and other insects which are harmful to grape vines has been adopted near Rheims. Posts supporting 5-candle-power electric lamps were placed in the vineyards, from each of which a dish, containing water with a top layer of petroleum, was suspended. During the first night these traps were placed in three parallel rows at distances of about 200 feet from each other, the distance between each lamp being about 75 feet. On the first clear evening late in July the current was turned on about eight o'clock, and the lamps remained burning until an hour or so after midnight. Soon after the lamps were lighted the insects swarmed toward them and were rapidly killed, either by the fumes of the petroleum or by the petroleum itself. The same operation was resumed the next clear night, but the lamps of the two outside rows were placed about 25 feet closer to those of the center row, and this was repeated in each of five subsequent clear nights, so as finally to bring the three rows within about 50 feet of each other. During the succeeding six or seven clear nights the movement was reversed in the same manner, so as to return the lamps to their position of the first night. As to the position of the lamps, numerous experiments were made during these trials, and it was proved that the greatest number of insects were killed when the petroleum dish was elevated only a few inches above the ground. These experiments were witnessed by representatives from a number of leading champagne makers, and this method was recommended to all wine growers who can avail themselves of the services of electricity.—Science.

TRADE NOTES AND FORMULÆ.

Syndeticon, Liquid Fish Glue.—Dilute 100 parts of concentrated fish glue with 120 parts of acetic acid and dissolve separately 20 parts of gelatine in 100 parts of water. Well mix both fluids and add 20 parts of shellac varnish to the mixture, stirring constantly.

Zapon Varnish.—Over 2 parts of colorless celluloid waste (obtainable at celluloid factories) pour 20 parts of acetone and allow the mixture to stand for several days in closed vessels with frequent stirring till the whole is dissolved to a clear, thick mass. Then add 78 parts of amylacetate and let the varnish settle for some weeks till it is completely clear.

Luminous Paint.—Twenty parts non-acid white gelatine, dissolved in 100 parts of water, to this add (dissolved) 3 parts chromate of potash and combine, with 10 parts of thickly liquid white lead or zinc white varnish, as pale as possible, by vigorously stirring into a homogenous mass. After thorough stirring incorporate 15 parts of a previously prepared phosphorescent powder.

Universal Spirit Varnish. — To 60 parts each of bleached shellac, crushed Manila copal and mastic and 15 part of Venice turpentine add 1,000 parts of 82½ per cent. alconol and some crushed glass and allow the mxiture to stand for 1 to 2 weeks, stirring frequently. Then add about 1 part of boracic acid and filter. The varnish may be used for metal, wood, paper, etc. In aniline colors soluble in alcohol are added to it, the so-called "brilliant varnish" (Brillantlack) is obtained, adapted for varnishing bottles, metal-plate and capsules.

Paint for Wooden Posts.—Fifty parts of rosin, 40 parts of finely crushed chalk, 500 parts of fine white and sharp sand; 4 parts linseed oil, 1 part natural red oxide of copper, 1 part sulphuric acid. First heat the rosin, the chalk, the sand and the linseed oil in an iron pot, then add the oxide, and, with great care, the sulphuric acid; mix all very carefully and by means of a stiff brush, apply to the wood while still hot. If the mixture does not appear to be sufficiently fluid, thin it with a little linseed oil. When this paint is cold and dry, it constitutes a coating as hard as stone, through which no moisture can penetrate.

Uralite is a fire-proof building material composed of pulverized asbestos wtih the addition of chalk, silicates, sulphuric acid, aluminium sulphate, etc. The compound is first pressed, dried and saturated with an adhesive and mineral colors, then pressed into forms, again dried and cut to the desired measure. It combines the advantages of stone with those of wood. It is fireproof; does not stretch; does not warp with heat or damp; can be nailed, glued or riveted; is a bad conductor of heat, electricity and sound, and is not sensitive to the action of acids, frost, or of cold and hot water. Its weight, it is true, is double that of oak, but it is said to be of peculiar excellence as a weather and fire-proof substance.

Zelodelite.—Under this title is recognized a mixture of sulphur and pulverized fragments of stoneware or glass. It is best prepared from 19 parts of sulphur and 42 parts of powdered stoneware or glass powder. The mass is heated until the sulphur melts when it is mixed by stirring and poured into molds. By this means we may make slabs that can be employed in place of lead for the construction of sulphuric acid cells, for the mass withstands the effects of air and acids, no matter how concentrated the latter may be. As it retains its solidity in boiling water and melts only at about 248 deg. F. it is well adapted for various other purposes for which we use asphalt; it even replaces hydraulic lime, as it cements stones together with the greatest tenacity. Where zeiodelite is used for the construction of sulphuric acid cells, in place of lead plates, great advantage is obtained, for the substance completely resists the acids. Although plates of 1½ centimeter thickness are used, while the lead plates are but 3 millimeters thick, its cost is but one-fourth that of lead plates and an acid, wholly free from lead, is obtained. To join the plates together they are set 2 centimeters apart and the joints filled with melted zeiodelite, heated to 636 deg. F. (300 deg. C.).

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